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OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

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EDITORIAL.

NOTICE.—The publication of the JOURNAL is made under the direction of the Editor and the Committee of Publication, who endeavor to exercise such supervision of its articles, as will prevent the inculcation of errors or the advocacy of special interests, and will produce an instructive and entertaining periodical; but it must be recognized that the Franklin Institute is not responsible, as a body, for the statements and opinions advanced in its pages.

The United States International Exhibition of 1876.—The prominent event of the past month has been the opening of the Centennial Exhibition, which occurred on the 10th.

The President of the United States, accompanied by the Acting Vice-President and the Cabinet officers, the Chief Justice and numerous Associate Justices of the Supreme Court, together with the Emperor of Brazil and the Diplomatic Corps of all nations, represented personally the executive government of the nation; while an almost complete gathering of the Senators and members of the House of Representatives was present in behalf of the National Congress; Governors and officials from most of the States of the Union completed the governmental delegation; the Army and Navy of the United States occupied conspicuous places in the procession; and civic and other officials, together with the Commissioners of the Exhibi-

tion and its officers, with other invited guests, were present—the entire number forming a body of about 10,000 persons, who participated directly in the opening ceremonies.*

Exhibitors and others who possessed the right of entry to the buildings, through which the opening procession was to pass, were spectators of the march, and an audience of about 10,000 more persons had collected within hearing of the speeches and music which preceded the event; while the returns of persons entering the enclosure, gave 85,000 more to be present on the grounds. A numerous body of soldiery from Massachusetts and Pennsylvania formed an escort for the President, and preserved the lines before and during the inaugural proceedings.

At 11 A.M., the President came upon the stand, and was received with great applause, and after an appropriate prayer by the Rt. Rev. Matthew Simpson, Bishop of the Methodist Episcopal Church of Philadelphia, and a beautiful memorial hymn by John G. Whittier,† and after receiving a brief address from Genl. Joseph R. Hawley, Presiding Officer of the Centennial Commission, in announcement of the completion of the preparations for the Exhibition; President Grant spoke as follows:

My Countrymen:—It has been thought appropriate, upon this Centennial occasion, to bring together in Philadelphia, for popular inspection, specimens of our attainments in the industrial and fine arts, and in literature, science, and philosophy, as well as in the great business of agriculture and of commerce.

That we may the more thoroughly appreciate the excellencies and deficiencies of our achievements, and also give emphatic expression to our earnest desire to cultivate the friendship of our fellow members of this great family of nations, the enlightened agricultural, commercial, and manufacturing people of the world have been invited to send hither corresponding specimens of their skill to exhibit on equal terms in friendly competition with our own. To this invitation they have generously responded; for so doing we render them our hearty thanks.

The beauty and utility of the contributions will this day be submitted to your inspection by the managers of this Exhibition. We

* The most noteworthy foreign guest present was Dom Pedro, Emperor of Brazil.

† An extraordinary musical and verbal rhapsody by Sydney Lanier followed Whittier's poem.

are glad to know that a view of specimens of the skill of all nations will afford to you unalloyed pleasure, as well as yield to you a valuable practical knowledge of so many of the remarkable results of the wonderful skill existing in enlightened communities.

One hundred years ago our country was new and but partially settled. Our necessities have compelled us to chiefly expend our means and time in felling forests, subduing prairies, building dwellings, factories, ship docks, warehouses, roads, canals, machinery, etc., etc.; most of our schools, churches, libraries, and asylums have been established within the hundred years. Burthened by these great primal works of necessity, which could not be delayed, we yet have done what this Exhibition will show in the direction of rivaling older and more advanced nations in law, medicine, and theology; in science, literature, philosophy, and the fine arts. Whilst proud of what we have done, we regret that we have not done more. Our achievements have been great enough, however, to make it easy for our people to acknowledge superior merit wherever found.

And now, fellow-citizens, I hope a careful examination of what is about to be exhibited to you will not only inspire you with a profound respect for the skill and taste of our friends from other nations, but also satisfy you with the attainments made by our own people during the past one hundred years. I invoke your generous co-operation with the worthy Commissioners to secure a brilliant success to this International Exhibition, and to make the stay of our foreign visitors —to whom we extend a hearty welcome—both profitable and pleasant to them.

I declare the International Exhibition now open.

After this address, the President, in company with the Director General of the Exhibition, Alfred T. Goshorn, entered the Main Building, at the head of a procession, and passed through it, crossing to Machinery Hall, where, at 1.15 P. M., the large "Corliss" Engine, which supplies motive power for the machinery, was put in motion by the combined efforts of Mr. G. H. Corliss (Commissioner and maker of the engine), the President, and Dom Pedro; and the opening of the Exhibition was consummated by the admission of the public generally.

Previous notices of the construction and progress of most of the principal buildings have been given in our JOURNAL, but for the purpose of record, it is here stated that there have been erected within

the enclosure about 190 edifices, of all sizes; of which the following list gives the area covered by the large ones:—

1. Main Building,	20·1 acres.	5. Memorial Hall,	1·5 acres.
2. Machinery Hall,	11·6 "	6. Women's Pavilion,	0·7 "
3. Agricultural Hall,	9·2 "	7. Horticultural Hall,	0·5 "
4. United States Hall,	2·0 "	8. Various Annexes,	5 to 6 "

9. Separate Buildings, about 150 in number, of which nearly one-half are mere stands. There are in all nearly 75 acres under roof.

The only unfinished buildings at the time of the opening were in the 8th and 9th classes of the above list, and are very generally edifices built by permission, for states or societies, and beyond the immediate control of the Centennial Commission. The grounds, however, are not yet in very good order. The Exhibition itself, on the day of opening, displayed much want of completion of arrangement, besides the numerous vacancies, where no attempt to open the packages had been made; in fact, a thousand tons of merchandise and machinery were then upon the cars, without the gates of enclosure; and as much more was known to be yet upon the seas, to arrive in New York or Philadelphia. Those, however, who had seen the grounds as late as the day before the opening, were much astonished at the completeness and degree of order which had been reached. Even the machinery exhibits, which seemed hopelessly confused at the last moment, were made to assume an appearance of finish and an actuality of starting into work, that was surprising. Since the opening, not quite so much energy has been expended in completion, as the past three weeks have not equaled the accomplishment of the three days ending May 9th; but at this time there remains little to be done in finishing the display. In some of the machinery annexes there are yet many machines not in order for examination; and in the agricultural department, the incompleteness is most marked; France, Russia, Turkey, and the Argentine Republic are behind in all branches of their show; but by the middle of June it can be anticipated that the noise of the hammer will cease upon the grounds.

The successful character of the Exhibition is already established beyond question; the variety, extent, thoroughness and beauty of the several exhibits, exceed the anticipations of the most sanguine of its friends, and far surpass the expectations of the general visitor. That a great share in this result is due to contributions from foreign nations, in which contributions art is shown in every form of beauty,

skill in every branch of industry, ingenuity in every application of mechanism, and intelligence in them all; with that total absence of political feeling that has made it possible for this national occasion, to become, in Philadelphia, a truly international exhibition.

In such an exhibition the only course is for the observer to examine and appreciate for himself that part of the show in which his calling, station or habit of life will give him particular interest. The best in the world is here, the best which can be had or seen, the most attractive show which the nation can make; the appeal to patriotism or to personal desire for profit or honor, has gathered all we can exhibit; foreign nations have entered into a friendly competition and comparison, and national pride and personal profit have stimulated them and their people to aid in their exposition of the progress of civilization; no American exhibition has at all approached this one in beauty or extent, and the success of the project is completed in the present accomplishment.

In the mechanical department of the Exhibition, in which the readers of the FRANKLIN INSTITUTE JOURNAL will have the largest interest, there will be some disappointment to many of those who expect to see novelties of process or production which will possess *popular* interest. It is too early to pronounce certainly that such do not exist; if they do they will become prominently notorious within a brief time; but the character of the mechanical exhibits of the past great exhibitions has gradually changed, until the only things shown freely are the products of manufacture, what people make to sell, and not the appliances for producing these products. And another result from the growth of mechanical industry is the advance from elementary discovery to technical improvement. Possibly a familiar example will convey to the reader the nature of this change. Any person tolerably informed in physics and electricity, who should in 1851, the date of the first great exhibition, have examined the electric telegraph and apparatus, could readily comprehend and popularly describe the whole so that the general reader or listener would be entertained and, possibly, instructed; but to-day, if a fully informed electrician and skillful mechanic were to visit one of our large telegraph offices, he would find apparatus complicated in construction to the last degree, diversified to unlimited extension, and involving in its principles of action the most subtle and abstruse problems, whose comprehension alone could only result from the

closest study or the most constant experience; and any description of accuracy, would fail to convey the least idea to the uninitiated, beyond the fact of the attainment of wonderful results. Similarly the sewing machine has now become involved in detail devoid of general interest to readers. And each development of former days, new and interesting at the time, when the general novelty has passed, has grown in particular interest as a technical subject, and lost otherwise.

The examination of the judges will bring out the most striking novelties in mechanism, and such as will prove interesting on these pages, will be gladly described and commented upon.

Proposed Canal between the Basin of the Caspian Sea and the Black Sea. Filling of the Basin of the Caspian Sea.—Mr. Henry C. Spalding, an American engineer, announces a project for the restoration of the ancient water level of the Caspian Sea to its condition in pre-historic times, by the cutting of a canal, some 170 miles in length, by which the waters of the Black Sea shall be drained into the basin of the Caspian. He has examined the country, and has prepared a report, which is intended to show that it is feasible to commence the work at some point in the basin where the natural surface is 50 feet below the Black Sea level, and to extend a channel westwardly, 500 feet in width, with a level bottom, until 35 feet of fall is obtained; from this terminus of a wide cut, he would continue the channel 15 feet in depth (below the Black Sea level) to some point on the shore of the Black Sea, where the canal would have 10 feet depth—the whole of this shallow canal being 150 feet wide. The ensuing flow of water (at the rate of about eight miles per hour would then be made, by proper direction and assistance, to excavate the remaining part of the earth, and the quantity of water, which would increase with the enlargement of the channel, he estimates would be sufficient in forty years to fill the basin to its capacity, while the enlarged channel would become navigable, and the great trade of Persia would be diverted into the new route. Mr. Spalding proposes further that there be formed a junction between the Don and Volga Rivers, and that the upper stream of the Don be diverted into the Volga, while the lower stream would be severed, and would drain from the Sea of Azof. This additional means of supplying the basin of the Caspian would reduce the time for filling it to twenty-five years.

A new Empire, with a new fertility, new people, and new climate, is anticipated to result from this geographical engineering.

Franklin Institute.

HALL OF THE INSTITUTE, May 17, 1876.

The stated monthly meeting was called to order at 8 o'clock, P.M., the Vice-President, Chas. S. Close, in the chair.

There were present 91 members and 12 visitors.

The minutes of the last stated meeting were read and approved.

The Actuary presented the minutes of the Board of Managers, showing that at their last stated meeting, 11 persons were elected members of the Institute. That the Scott Legacy Premium and Medal were awarded to Morris L. Orum, for his flexible mandril for bending metal pipes; that the President has appointed the following persons members of the Centennial Reception Committee:—J. E. Mitchell, *Chairman*; Hector Orr; H. Cartwright; W. Helme; J. J. Weaver; L. M. Haupt; J. M. Goehring; Dr. G. M. Ward; S. Lloyd Wiegand; G. Morgan Eldridge; J. W. Nystrom; J. S. Bancroft; Dr. C. M. Cresson; Wm. M. Henderson; H. R. Heyl; S. R. Marshall; C. E. Smith; C. S. Heller; C. Sellers, Jr.; R. Estrada; W. H. Thorne; Thos. S. Stewart; C. Chabot; D. D. Willard; W. F. Durfee; A. G. Buzby; Prof. E. J. Houston; Samuel Sartain; and that the following donations were made to the Library:

Navy Register of the United States to January 1, 1876. Washington, 1876. From the Navy Dept.

Report of the Board to recommend a standard gauge for bolts, nuts, and screw-threads for the United States Navy. May, 1868. Washington, 1875.

Extract from annual report of the Chief of Bureau of Steam Engineering to the Secretary of the Navy, Oct. 25, 1867. Washington, 1870.

Report of the Trials of the Steam Machinery of the U. S. Revenue Steamers Rush, Dexter, and Dallas, at the U. S. Navy Yard, Boston, Mass., in the month of August, 1874.

Reports of the Chief of Bureau of Steam Engineering for 1871 and 1872, with appendix. Washington, 1873.

Annual Report of the Bureau of Steam Engineering. Washington, 1873–4–5. From the Bureau of Steam Engineering.

Notes for the Guidance of Inventors, being part of a series of Articles by W. Lloyd Wise. Reprinted from Engineering. London, 1875. From the Author.

Burley's U. S. Centennial Gazetteer and Guide, 1876. Philada. From S. W. Burley, publisher.

Officiller Bericht über die Sachsische Gewerbe und Industrie-Ausstellung zu Dresden, 1875. From W. H. Uhland, Leipzig.

Centennial meeting of the Philadelphia Contributionship for the Insurance of Houses from loss by fire. Philada., 1852. From the Philada. Contributionship, &c., April, 1876.

Program der Grossherzoglich Badischen Polytechnischen Schule, zu Carlsruhe für das Studienjahr, 1875–76. Carlsruhe, 1875. From the Polytechnic School.

On the Physical Geography of the part of the Atlantic, which lies between 20° N., and 10° S., and extends from 10° to 40° W., by Captain Toynbee, F.R.A.S., &c. London, 1846. From the Meteorological Committee.

The Magic Square, containing all numbers from 1 to 10,000, so arranged that counting perpendicular, horizontal, or diagonal, every line will count the same. Arranged and presented by Joseph Nicholson, Philada., Penna.

Annual Report of the Supervising Architect to the Secretary of the Treasury for 1875. Washington, 1876. From Wm. A. Potts, Superv. Arch't.

Sixth Annual Report of the President and Directors of the Lake Shore and Michigan Southern Railway Co., to the Stockholders for the Fiscal year ending Dec. 31, 1875. From the Company.

List of Members of the Institution of Mechanical Engineers. From the Institution.

Report of the Tenth Industrial Exhibition, under the auspices of the Mechanics Institute of the City of San Francisco, held between August and Oct., 1875. From J H. Culver, Secretary.

Specimen Fasciculus of a Catalogue of the National Medical Library, under the direction of the Surgeon-General U. S. A. at Washington, D.C., 1876. From J. S. Billings, Assist Surg. U. S. A.

Contributions to the Natural History of Kerguelen Island, made in connection with the United States transit of Venus expedition, 1874–75, by J. H. Kidder, M.D., II. Washington, 1876.

Bulletin of the U. S. National Museum. No. 5, Catalogue of the fishes of the Bermudas, by G. Brown Goode. Washington, 1876.

Annual Report of the U. S. Geological and Geographical Survey of the Territories, by F. V. Hayden. U. S. Geologist. Washington, 1876. From the Dept. of the Interior.

The Actuary also reported the following, adopted at a special meeting held April 25th :

Whereas, This Board has been informed of the death of Geo. Washington Smith, Esq., a venerable and esteemed citizen of Philadelphia,

prominent in philosophy and science during a long life, and a member of the Franklin Institute. Therefore

Resolved, That we recognize him as one of the original friends and founders of the Franklin Institute, who followed us with his works and advice to the completion of our first half century; that though acknowledging the favor of this long service, we present our deep regret at this, his late and final withdrawal from our fellowship.

Resolved, That we recommend the attendance of members of this Board at his funeral, as a testimony of our respect and sorrow.

Resolved, That a copy of these proceedings be presented to the family of Mr. Smith, and also reported to the Institute at the next meeting.

Mr. Hugo Bilgram then read the paper announced for the evening, being on "The Temperature of the Sun."

The Secretary presented his report, embracing Appleby's Door Mat Alarm; Curtis' Regulating Gas Burner; Donghoue's Anti-incrustation Battery for Steam Boilers; Eccle's Quadruple Screw Press; a Book Rest for Invalids, the invention of Mr. John McClure; specimens of two heavy Turning Tools made from scrap steel, and sent by Mr. Chas. Graham, of the Bloomsbury division of the Delaware, Lackawana and Western R. R.; and a Crook's Radiometer, made by Mr. J. J. Hicks, London.

Under the head of deferred business the subject of the metric system of weights and measures was taken up.

Mr. Briggs, through the Secretary, withdrew the extended minority report, which was partially read at the last meeting, and presented a shorter one which was read.

Mr. W. Jones moved that the majority report be adopted, and that the Secretary be directed to transmit a copy of the same to the Boston Society of Civil Engineers.

Mr. Orr offered the following as a substitute, which, on motion, was accepted, and after much discussion, the resolutions were adopted.

Resolved, 1st, That the two reports of the committee, relative to the metric system of weights and measures be accepted and filed, and the thanks of the Institute be expressed for the same.

Resolved, 2d, That while we decline to ask for, or favor compulsory legislation to enforce a change in our weights and measures at this time, we believe that the adoption of a harmonious system of money, weights and measures, throughout the bounds of mercantile intercourse is most desirable; and that we stand ready to do our part in the shaping of such a system, and in the application of the same when it has been devised.

Mr. Jones then moved that the majority report be adopted, and a copy thereof be sent to the Boston Society of Civil Engineers, and that both reports be printed in the JOURNAL.

Mr. Bary moved to divide the subject so as to make the matter of printing in the JOURNAL a separate resolution, which was carried.

Mr. Chabot moved to still further divide Mr. Jones' motion, so as to make the question of the adoption of the majority report one resolution, and the sending of a copy to the Boston Society of Civil Engineers another resolution.

Pending the discussion of Mr. Chabot's motion, the further consideration of the subject was, on motion, postponed to the next stated meeting.

On motion, the meeting then adjourned.

J. B. KNIGHT, *Secretary.*

REPORT

To the FRANKLIN INSTITUTE of the State of Pennsylvania, for the promotion of the mechanic arts:

The Committee to whom was referred the circular of the Boston Society of Civil Engineers, asking the co-operation of the Institute "in petitioning congress to fix a date after which the metric weights "and measures shall be the only legal standards," respectfully report:—

The subject of weights and measures, which are *the instruments used in weighing and measuring*, has received the attention of all governments, and always with a desire to promote uniformity. The literature of the subject is copious.

Your committee will not repeat it, except so far as may enable us to determine the propriety of the proposed action. They refer to the admirable report of John Quincy Adams, Secretary of State, made to congress Feb'y 22, 1821,* and to the reports of the Board

* In Mr. Adams' journal, Feb'y 22, 1821, after speaking of sending to the Senate, the treaty with Spain for the cession of Florida, he says:

"I sent at the same time to both houses, the report upon weights and measures prepared conformably to a resolution of the Senate of March 3, 1817, and one of the House of Representatives, of Dec. 14, 1819."

"And thus have terminated, blessed be God, two of the most memorable transactions of my life."

. . . Of the report he says: "It is, after all the time and pains I have bestowed upon it, a hurried and imperfect work; but I have no reason to expect that I shall

of Managers of the Franklin Institute, made to the legislature in this state in 1834, published in the JOURNAL of that year, for the history of the subject and to the various encyclopedias for information as to the present state of metrology in France.

We are invited to adopt the system of France and to compel our people to use it. With her example before us as a guide, we may contrast with our own :

1. The condition of the weights and measures of France before her revolution.

2 The opportunity presented by the times when the French undertook their change.

3. The character and habits of the French government and people.

4. The system as originally designed by the French commission and as ordained by law.

5. The passive resistance of the people to the changes, the entire rejection and abolition of parts of the system (including all compulsory provisions), the compromise of 1812 in the adoption of the *système usuel*, in combination with the decimal metrical system originally forced upon the people, and the final establishment of this system in 1840.

6. The reasons for this resistance and reaction. And then, after a consideration of the immense number of fixed and recorded measurements now existing in this country, and of the expense, labor, and confusion which the attempt to change them would occasion, we shall perhaps be able to form an opinion as to the wisdom of such an attempt :

1. The condition of the weights and measures in France when the Bishop of Autun proposed a reform, may be described as legal confusion. In the memoir of the Bishop (afterwards Prince de Tallyrand) he enumerates 13 different lengths of the foot (*Pied*) in legal use, measuring from 120 to 150½ *lignes ;* 18 different legal yards

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ever be able to accomplish any literary labor more important to the best ends of human exertion, public utility, or, upon which the remembrance of my children may dwell with more satisfaction."

The report is republished in Davies' Metrical System, Barnes, N. Y., 1871. It is a philosophical and judicial investigation of the subject, candidly stating the advantages and disadvantages of the various systems. Quotations from it may be made favoring either side.

(*Aunes*) measuring $299\frac{80}{100}$ to $597\frac{20}{100}$ *lignes;* 21 different legal pounds (*Poids de Marc*) weighing 6479 to 9767 grains; 24 legal *Boisseaux*, containing from 128 to 5157 cu. in.; 17 legal *sacs*, containing 3584 to 7349 cu. in.; 23 legal *septiers*, containing from 924 to 19,830 cu. in., 13 legal *Tonneaux*, containing from 12,203 to 97,989 cu. in., besides others, and adds, "this table is only a much abridged extract "of the principal differences between the weights and measures of "the kingdom."

The situation in France invited reform. There is no such diversity here. The weights and measures are by law, uniform throughout the United States, although slight diversity may exist in fact. The same motive for change does not exist here as existed in France.

2. The opportunity presented to France was peculiarly favorable for a change. The new system was struck out during the red heat of the revolution, during which the king was dethroned and beheaded, the nobles were killed wherever found, and their property, when they fled for their lives, was plundered or confiscated. The priests were driven from their cures, and religion was abolished. The Christian era disappeared and the world began again at the year 1 of the French Republic, upon the 22d of September, 1792, "the day of the autumnal equinox, when the sun entered the sign of "the balance, the symbol of equality."

However, favorable to the introduction of a perfect system of weights and measures, your committee hope that no such opportunity may be presented in this country.

3. The government of France has always been in the habit of interfering with the private affairs of the people. For instance: the prices of butchers' meat and of bread are fixed by the prefects of police. A journeyman may not engage with a master mechanic without the permission of the same officers, etc., etc. The people are accustomed to this parental care, and would feel lost if it were withdrawn. They will, if necessary, rise up and destroy the government, but while it *is* the government, they are accustomed to feel its hand in their private affairs. A law, which proposed to abolish the old weights and measures in use, and the old habits of the people in weighing, measuring, and trading, and to substitute new ones with new names, would be more likely to be obeyed when enacted by the government of France than by that of the United States.

This government is authorized by the constitution to "fix the standard of weights and measures." It would be questioned whether this power "to fix," meant to change, to abolish, and to substitute new and foreign weights and measures. Our general government has never undertaken to enforce the laws to maintain the existing standards. The supervision of the subject has always been the care of the separate States. A law of the United States, such as proposed, would probably be a dead letter unless enforced by means which the people would not submit to. The American idea of government duties is, that it should do and enforce justice, and that Liberty in all things innocent, is the birthright of the citizen.

4. The system established by the French, and the difficulties of the undertaking, may best be understood from a brief chronological sketch.

In 1790, Talleyrand addressed a memoir to the constituent assembly, setting forth the condition of the existing metrology and proposing to establish a new system for all France, whose primary unit should be the length of a pendulum beating seconds, as a natural standard. His proposition did not embrace a decimal system.

A decree, adopting the proposition, but with serious modifications was sanctioned by the king, Louis XVI, upon the 22d of August, 1790.

In execution of this decree, a committee of the Academy of Sciences was appointed to examine and report upon the subject. The report was made August 19th, 1791. It proposed the ten millionth part of a quadrant of the meridian as a natural standard unit of lineal measure to be applied as a measure of matter in its three modes of extension, length, surface and solidity; and as a secondary standard of comparison with this unit, the length of a pendulum vibrating one hundred thousand beats a day. The weight of distilled water contained by a cubical vessel in decimal proportions to the lineal measure, was to be the standard unit of weight. The whole system of weights and measures was to be composed of multiples and subdivisions of these units according to the decimal system. The report recommended that the quadrant of the arc of the meridian should be divided into 100 degrees instead of 90, as before. The decimal division of time also was part of this plan.

To carry this plan into effect it was necessary, with the utmost accuracy, to measure the arc of the meridian, to weigh the ascertained

bulk of water, and to find by experiment, the length of the pendulum beating 100,000 seconds per day. The report being sanctioned, the execution of the scientific observations was immediately begun, but was not completed before seven years, owing to numerous interruptions, occasioned by the overthrow of the government and the abolition of the Academy of Sciences.

Upon the 5th of October, 1792, the new calendar was established by law. It made 100 seconds in a minute, 100 minutes in the hour, 10 hours in a day, 10 days in a week, 3 weeks in a month, and 12 months in a year. Thus 100,000 seconds made a day, 30 days made a month, and 360 days a year. The five or six odd days in the natural year, having no month to cover them, were called in derision *Sans Culottides*, or, days without breeches, and were devoted to games and frolics.

The quadrant of the circle was also divided decimally into 10 parts and each part into 10 degrees. The quadrant of the meridian containing 100 degrees of 100,000 *metres* each, was to be 10,000,000 *metres*, and the circumference of the earth, forty millions of *metres* in length.

The universe, under this system, might be compared to a great French clock, having the earth for its escape wheel, whose equatorial motion would be 400 *metres* per second.

The National Assembly, impatient at the delay in establishing the definitive *metre*, decreed upon the 1st of August, 1793, that the system should go into operation immediately, based upon a measurement of a degree of the meridian made in 1740, which made the length of the *metre* $443\frac{44}{100}$ *lignes* of the ancient French foot. This decree adopted a complete nomenclature of all weights and measures for lines, surfaces and solids. The length of the *metre*, and values of all measures derived from it, were to be provisional and lawful until the final determination of the correct figures. The new nomenclature and the use of 100,000 seconds per day, were made compulsory by the law of 24th November, 1793.

By the law of 7th April, 1795, some of the names were changed and the existing nomenclature of the French metrology was *definitively* established, although the sizes of the weights and measures were *provisional* only. This law provided that the weights and measures might be made of the units, ten units, the double units, half units, and tenth units, but no other multiple or subdivision, such as

$\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{6}$, $\frac{1}{8}$, should be made or used. The same law re-established the scientific commission for the work of determining the definitive *metre*, etc., and repealed so much of the law of 24th November, 1793, as obliged people to use 100,000 seconds in a day.

The arc of the meridian between Dunkirk and Barcelona was measured by Messrs. Delambre and Mechain, with great accuracy. It comprehended about $9\frac{2}{3}$ degrees of latitude. The measurement was continued south by Messrs. Borda and Biot to the island of Formentara, so as to comprehend 12 degrees of latitude, of which, 6 were south of the 45th parallel, and 6 north of it.

The degrees of latitude were found to be of different lengths and the differences followed no law. An average was therefore taken, and then the length of the *metre* was determined to be $443\frac{94}{1000}$ *lignes* of the old French foot (being $\frac{1}{100}$ of an inch less than the provisional *metre*) and equal, according to the measurements of the French Academy, to 39·3827 in English; of Captain Kater (English), 39·37079; of Mr. Hassler (U. S. Coast Survey), 39·3802. Doubts have been thrown upon the correctness of all these measurements.

The length of the pendulum vibrating 100,000 seconds per day at Paris was found to be ·74193 *metres* (29·2192 in.), from which was afterwards deduced by calculation, the length of the pendulum vibrating the usual seconds of 86,400 a day, equal to ·99383 *metres* (39·1393 in.) or $440\frac{56}{100}$ *lignes* of the old French foot.

The weight of distilled water contained in a cubic *decimetre* was found to be $18{\cdot}827\frac{15}{100}$ grains French, equal to $15{\cdot}445\frac{72}{100}$ grains troy, which is the weight of the *kilogramme*.

The capacity of the vessel containing this water is the capacity of the *litre*, which is equal to 61·02624 cubic inches. This is the standard unit for wet and dry measure.

The principle of decimal arithmetic was applied to all these units. The multiples were tenfold, and the subdivisions were tenths, so that in any sum representing French measures, each figure has ten times the value of its right hand neighbor. To all the multiples of the system, Greek words are prefixed, and to all the subdivisions, Latin words.

The actual standard measures of the *metre* and the *kilogramme* were deposited with the keeper of the public archives, with great form and ceremony, upon the 22d of June, 1799.

The temporary weights and measures provided by the law of August 1st, 1793, were abolished and the definitive substitutes were established by law upon the 10th of December, 1799.

The establishment of the French decimal metrical system has thus far been described. The process of modification and repeal of compulsory measures followed. We have already seen that the law of April 7th, 1795, repealed the compulsory use of the second of $\frac{1}{100,000}$ of a day. This was a revolt against "decimal despotism."

On the 8th of April, 1802, a law was made, retaining the republican calendar for civil purposes, but restoring the week of seven days and the old Sundays.

By the law of November 23d, 1802, the wine trade was relieved from the compulsory use of the new system, which required that the casks should contain a decimal number of *litres*. It was now permitted that the casks might be made of the ancient sizes and the contents in *litres* branded upon them. In the newspapers at Bordeaux the prices current at this day are quoted by the *tonneau* of 4 *bariques*.

On the 9th of September, 1805, the new calendar (after an existence of twelve years) was abolished, and the ancient calendar was restored, so that January 1st, 1806, reappeared.

On the 12th of February, 1812, an imperial decree, executed by an ordinance of 28th of March, following, abolished the compulsory provisions of the decimal system so far as to permit the use, for the purposes of commerce, of the following weights and measures:

Lineal measure.

Toise = 2 *metres*, divided in 6 feet. On one side divided into *decimetres*, and the first division into *millimetres*.

Pied = $\frac{1}{3}$ *metre*, divided in 12 inches, each inch in 12 lines. On one side divided into $3\frac{1}{3}$ *decimetres*, and subdivided into *centimetres* and *millimetres*.

Aune = 1·2 *metres* divided into $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, $\frac{1}{3}$, $\frac{1}{6}$, $\frac{1}{12}$. On one side marked in decimals of *metres*.

Capacity.

Boisseau = $12\frac{1}{2}$ *litres*, also its double, half and quarter.

Litre = also its subdivisions of $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ for retail sales of wet and dry measure.

Weight.

Livre = $\frac{1}{2}$ *kilogramme* or 500 *grammes* = 16 *onces*, also subdivisions of $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$ths.

Once = 31·25 *grammes* = $\frac{1}{16}$ *livre*, also subdivisions of $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$.

Gros = 3·90625 *grammes* = $\frac{1}{8}$ *once*.

Thus after twenty years of contest and confusion, the old names and the old subdivisions were restored, but with new and uniform values.

This led to many years of confusion and fraud, followed by another change, decreed by the law of July 4th, 1837. This law was executed by the royal decrees of April 17th and July 10th, 1839, and put in operation in 1840, when the existing decimal metrical system was finally established, after a struggle of 47 years, a period of two generations, during which the entire active population of France was changed.

The preceding history is given to show the extreme difficulty of effecting a change in the weights and measures used by the people, even when a reform was most needed, because of the greatest confusion and diversity of the weights and measures in use, and when the change was enforced by the most bloody and arbitrary despotism of modern times, favored by the best of opportunities.

Although the *metre* was drawn from the circle and the sphere, these two forms resisted the application of the decimal metrical system. The measurements of time, of the degrees of the circle, of navigation, geography and astronomy, successfully rejected it, although the prime idea of the Commission was to connect these subjects with ordinary weights and measures, by making the *metre* (the 40 millionth part of the circumference of the earth), *the unit of lineal measure*, and the *second* (the hundred thousandth part of the day) the *unit of time*, by means of the pendulum beating 100,000 seconds. The *metre* and the *second* were then the intermediate links in a long chain connecting science and practical life, having the solar system at one end, and a quart measure at the other. It is singular that the parts of this chain applicable to the calculations of science, were at once abandoned for their inconvenience; and the parts applicable to the uses of yard sticks, pound weights, and quart measures, were imposed upon the people by compulsory laws for nearly twenty years, without regard to the still greater inconvenience to them.

Excuse for this partiality may be found in the facts, that the division of the day into 86,400 seconds, and of the quadrant into 90 degrees, was uniform throughout France, and throughout the world; that, although the day and the quadrant were not decimally divided, they were conveniently divided according to the nature of things; that there was a great value invested in the clocks and watches, and instruments for measuring time and circles, and in the tables and

calculations already made for the purposes of navigation, mensuration and astronomy, which would become useless if the changes were persisted in.

In fact, there were all the reasons for not making these changes, which we have now against the changes proposed to us; and there was no stronger motive. These reasons prevailed, and these changes were abandoned.

From these remarks we may infer why the French people resisted a reform conferring such benefits upon the nation, and perfecting its unity. Of course, the first objection was that it was a change; a change which required them to unlearn much of their little learning, to abandon many of their old customs, and to embrace new things with outlandish names. The philosophers laughed at this reason, but they yielded to it themselves.

If this objection had existed alone, the strong hand of the government—persisting for twenty years—must have conquered it. But there were other and more enduring reasons. The new system was not so perfect as to be in all cases preferable to the old. The usual divisions and subdivisions of weights and measures are the result of the natural selection of thousands of years, and they are in harmony with the daily wants and usages of practical life, requiring divisions of quantities into halves, thirds, quarters, sixths, and eighths, not always convenient in decimals.

But whatever were the controlling reasons which incited the opposition to a change in France, they have much greater force with us from the absence of motive. We have no such confusion and diversity as the French had, and no such reform is called for. Our money is already decimally divided, and we enjoy already the chief benefits which the new system gave to the French.

If the measurements of the weights and the dimensions of substances, when ascertained, were only to serve as data for complicated calculations, the reasons for adopting weights and measures decimally divided, would have controlled the practice long ago. This is actually the case with us; in surveying land, which is measured by chains twenty-two yards long, divided into one hundred links; in civil engineering, when embankments, excavations, etc., are measured by yards and tenths, or feet and tenths, as the case requires; in the measurements of ships for tonnage, when the three dimensions are taken by feet and tenths; and in gauging casks, which is done with a gauging rod marked in inches and tenths.

But the fact is, that the vast majority of weighings and measurings are followed merely by mental calculations; or, by a simple multiplication of quantity (whole or fractional) by price (in decimals), a process which can oftener be done by vulgar fractions, more easily than by decimals.

The *metre* is really as arbitrary a standard as the foot. About 80 degrees of latitude have been measured, but no two of them have been found of the same length, and there is good reason to believe that the length is not permanent in the same place. The only real thing about it is the rod in the public archives. The length of the *metre* is to be recovered, if lost, by comparison with the length of the seconds pendulum, and so likewise is the length of the foot or yard.

The *metre* was adopted in France for the lineal unit, in preference to the length of the seconds pendulum, only because the harmonious proportion between the *metre* and the length of the meridian would bring all local measurements into harmony with the measurement of the world, and would be a great assistance in geography and navigation; but the decimal divisions of the quadrant and of time, having been abandoned, and the adopted length of the *metre* having been found incorrect, there remains not even a sentimental reason for our adopting it as our unit of measure. Our own convenience should be our guide, and overwhelming reasons forbid us to incur the confusion, labor and expense of attempting to make a change of that kind.

In the opinion of your committee, the *metre* in any shape heretofore adopted, is a less convenient instrument for measurement than a two foot rule. You cannot fold it into four without breaking the sub-units. If so folded, it would be ten inches long, which is inconvenient for the pocket. The *metre* is only decimally divided, whereas the foot rule, besides being divided into tenths and hundredths, is also divided into twelve inches, and gives the even $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{10}$, $\frac{1}{12}$ and $\frac{1}{100}$ of the foot, and the $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, $\frac{1}{6}$, $\frac{1}{8}$, $\frac{1}{10}$, $\frac{1}{12}$ and $\frac{1}{16}$ of the inch.

By changing our unit of lineal measure for the sake of uniformity with France, we should sever our uniformity with Great Britain, a country with which three-fifths of our foreign commerce is transacted.

The change in our units would entail much greater expense than is usually imagined. The measurements of every plot of ground in the United States have been made in acres, feet and inches, and are

publicly recorded with the titles to the land, according to the record system peculiar to this country. Hundreds of years would elapse before we could permit ourselves to forget these old measures. Besides this, the industrial arts during the last fifty years have acquired a far greater extent and precision than were ever known before. Take, for instance, the machine shops, in which, costly drawings, patterns, taps, dies, rimers, mandrils, gauges and measuring tools of various descriptions for producing exact work and repetitions of the same with interchangeable parts, are in common use.

It has been calculated that in a well regulated machine shop, thoroughly prepared for doing miscellaneous work, employing 250 workmen, the cost of a new outfit adapted to new measures, would not be less than $150,000, or $600 per man.* If, instead of changing the sizes, we adopt the alternative of giving the French dimensions to the old sizes, the irreconcilable discord between the inch and the divisions of the *metre*, would furnish a precious example of the simplicity of the decimal system.

If new weights and measures are to be adopted, all the scale beams in the country must be regraduated and readjusted; the thousands of tons of brass weights, the myriads of gallon, quart and pint measures, and of bushels, half bushels and peck measures, and every measuring rule and rod of every description throughout the land, must be thrown aside, and others, which the common mind cannot estimate, must be substituted.

The great mass of English technical literature would become almost useless, and must be translated from a language which we, and the nation we have most to do with, understand perfectly, into a new tongue, which is strange to most of our people. As a question of cost, let those who advocate this change consider it carefully.

To the teacher, to the closet scholar, to the professional man, to those who never handled a rule or a measure, but only use weights and measures in calculation, it may seem merely a matter of legal enactment; but to the worker, the dealers in the market places, to those who produce the wealth and prosperity of the land, the question is a most serious one.

The Franklin Institute has never placed itself on record as opposing true progress; it has always advocated changes which were

* See "The Metric System in our workshops, &c.," by Coleman Sellers—JOURNAL OF THE FRANKLIN INSTITUTE, June, 1874.

beneficial and not destructive. In this case, a majority of your committee believe that the ultimate benefits of the change proposed, would be of less value than the damages during the transition. They think that the government of the United States has already done all that can fairly be asked of it by the most enthusiastic advocate of the metrical system, by making it legal. Those of us who choose to do so, can use that system, and no one can object to it; but, for the government to require us to use that, and no other, would be an arbitrary measure which we are neither willing nor able to bear.

The majority of your committee are of opinion, and so report, that the objections to the attempt to adopt the *metre* as a standard unit of lineal measure, are overwhelming, whether we consider the compulsory means proposed, or the end to be attained.

All of the objections to the metrical decimal system do not apply to the adaptation of the decimal scale to our existing units. In the decimal harmony between the cubic foot and its content of water weighing 1000 ounces avoirdupois, whereby a cube of $\frac{1}{10}$ of a foot on the edge becomes the measure of the ounce of water, we have the means of constructing a decimal system of weights and measures which would interfere the least with existing institutions. But your committee do not feel called upon to consider this branch of the subject.

COLEMAN SELLERS.
W. P. TATHAM,
Chairman.

Philadelphia, April 19th, 1876.

REPORT

To the FRANKLIN INSTITUTE of the State of Pennsylvania, for the Promotion of the Mechanic Arts.

The undersigned, the minority of the Committee of three to whom was referred the circular of the Boston Society of Civil Engineers, asking the co-operation of the Institute "in petitioning Congress to fix a date, after which the metric weights and measures shall be the only legal standards;" respectfully reports: That while he agrees with the conclusions of the majority of the committee, so far as relates to the subject specifically referred to them, that it is inexpedient

to attempt at present to anticipate, by enactment, the time when this great step in the progress of human civilization and unity, shall be taken by the national government of the United States; he does so solely upon the grounds of the yet incomplete preparation and education of the people; and their want of appreciation of the immense advantages in the progress of the arts, and in the applications of science which the metric system presents.

It having been decided by the Board of Managers that it is inexpedient to publish, in extended form, the statements and arguments which have been prepared by the minority of the committee in support of his views, it is therefore considered by him most proper to restrict this report to a simple protest against the tenets and assertions of that of the majority, as already published and circulated.

The undersigned protests against the perversion of history set forth in the majority report; wherein the struggle by which an uneducated, and at the time, lawless people emancipated themselves from the trammels of their hereditary customs, and acquired a uniformity in the system of weights and measures, is made to appear as a defect in the result finally attained. The perversion of history, which sets forth as examples what transpired from 1793 to 1812, couples with it an unwarranted assertion as to what transpired from 1812 to 1840, and ignores altogether everything which has occurred in the progress of nations during the past thirty-six years. The perversion of history that looks back to the school-boy days of our older active members, and with the repugnance to change of the most conservative old man of 1840, accepts the conclusions that "The usual divisions and sub-divisions of weights and measures are the result of the natural selection of thousands of years, and they are in harmony with the daily wants and usages of practical life, requiring division of quantities in halves, thirds, quarters, sixths, and eighths, not always convenient in decimals." To an American of any position in life, the absurdity of this allegation, (from which, by strict analogy of reasoning, it would follow, that the old £ s. d. were superior to our decimal coinage), has only to be stated to be appreciated; and to the Frenchman of like social condition, the application of this paragraph to the metric system might provoke merriment, but it could not appear to need serious reply.

The undersigned protests against the statements as to the convenience, suitability, or permanency of our present complicated and

diverse measures and weights—those of land or other lineal measures—the bushels, pecks, quarts—the gallons, quarts, pints—the pounds, ounces, grains—and especially against the averment that the "common mind" can estimate these irrational and disconnected magnitudes more perfectly than those which have simple and definite multiples and relations. The undersigned also protests against the whole line of argument which is, or may be, based on the assumption of great or immediate cost which will accompany or follow the introduction of the metric system; and particularly that excessive cost will ensue in the machine shops, where men work to gauges only, and where measures of length are unknown, except for computation.

Above all, and finally, the undersigned protests against the entirely erroneous assumption of the majority of the Committee, that it is "the teacher, scholar or professional man, who desires for himself the introduction of the metric system"—the truth is completely the reverse. To the teacher, the scholar, or the professional man the introduction of the metric system is of the least consequence. Those who have the habitude to use figures freely in computation, whose avocations keep their minds always awake to the relations of magnitudes of various orders and to the physical units of matter, are not embarrassed nor burdened seriously by the use of a few figures over and above what are absolutely requisite. It is the practical man only, who will reap the great advantage from the simplification of processes of estimates, which result from the metric system. His language of computation will then become in notation, that of ordinary figures, while, whatever his calling or business, he will find himself in possession of the key to the figures, dimensions or magnitudes of all other pursuits. He will be made free to apply for himself those studies of the practice of the arts which we call sciences, and he will be able to see intelligently the results of the practice of others in his own land and country, and by the spread of the metric system, that of all other nations.

The universal introduction of the metric system is merely a question of time. Within a century probably, it will be established in our own land. Possibly another century may pass before its complete adoption is consummated. Upon the progress of education mainly, but partly upon intercourse with other countries using the system, the period of time before which our people will come to understand the advantages to be gained, will be less or more remote; but sooner or later our people will voluntarily impose upon themselves the learn-

ing of how to use the metric system. Its use for all purposes of computation will follow almost instantly, and your committee believes, without demand for so stringent compulsory enactment as has recently been enforced in Germany.

That the Franklin Institute should carefully preserve its record in the cause of the advancement of science and the arts, is a point on which its members should feel the greatest interest, and exercise the most watchful solicitude. Its advocacy and support to all measures of improvement, however remote the end, should be asserted and maintained at all events, and the acceptance of the report of the majority as an expression of the views of the Institute as a body, should be carefully avoided. If, in the rivalry of kindred societies, we neglect to avail ourselves of one of the steps, which, more than all others, goes to smooth the road to learning; one which pre-eminently offers, after a few months' training, to the grown workman, or by the preliminary education of the child, who is to become a workman, the facility to use and comprehend the figures and computations of his calling; one that simplifies and reduces the quantity of arbitrary learning, which has so embarrassed human progress in past ages; if we, of the Franklin Institute, neglect, or fail to assert ourselves in behalf of advancement, we will be surpassed and displaced in the reputation we have hitherto supported and maintained.

Still, while the minority of your committee is firmly of opinion that the Franklin Institute should *favor* to the utmost, the immediate introduction and use of the metric system, he is free to admit that the time has not yet been reached, when the people are prepared for enforcement, either by voluntary or legal action; and in view of this fact he offers the following preamble and resolutions for the consideration of the Institute. Signed,

ROBERT BRIGGS,

Minority of Committee on compulsory introduction of the metric system.

PREAMBLE.

WHEREAS, Sufficient time has not elapsed since the passage of the act of Congress, giving permissive lawful use to the metric system, to exhibit the progress of the adoption and employment of the same, or to allow any definite opinion as to the future of the introduction of the system in the United States, and

WHEREAS, A compulsory law ought only to be based upon popular requirement and urgent necessity; therefore,

Resolved, That, in the opinion of the Franklin Institute, the time has not yet come, when an enactment by Congress should fix the date for compulsory use of the metric system.

Resolved, That the Secretary be requested to communicate this action of the Franklin Institute to the Boston Society of Civil Engineers.

COMMUNICATION.

To the Editor of the JOURNAL OF THE FRANKLIN INSTITUTE:

I beg leave to offer to the JOURNAL the following abstract of my remarks, in the discussion of the reports of the Committee upon the compulsory use of the Metric System, made at the stated meeting May 17, 1876.

Yours,

JOHN W. NYSTROM.

Upon the subject now under discussion—the acceptance by the Franklin Institute of the majority report which has been made to it—I wish to remark:

The majority report has been printed and circulated in pamphlet form, as if approved by the Institute, and is opposed to recommending the adoption of the metric system in this country; to which opposition of the committee, I have no objection: but before that report is adopted by the Franklin Institute, it is desirable that it should be based upon tenable ground, and not uttered in that spirit of depreciation of the metric system, and of the French nation, which seems to have inspired the Committee. That nation deserves great consideration for its struggle to introduce a universal system of metrology; an enterprise which, although universally desired, no other nation has ventured to undertake.

The majority report expatiates upon objections to the introduction of the metric system in this country, which are of mere temporary and insignificant import, very much like the English objections to the introduction of the Arabic figures for the Roman notation some 300 years ago. The English were about 400 years behind the Continental nations in the introduction of our present Arabic digits. The English thought that the introduction of the Arabic figures for the Roman notation, would obliterate all records and reckoning, and they expatiated upon the great difficulty and expense in making the alter-

ation. Now, the majority report on weights and measures to the Institute, is conceived in the same spirit, in regard to the introduction of the metric system. What would our technical books, our arithmetic, reckoning and records be to-day with the Roman notation?

At the April meeting of the Institute, it was remarked that the majority report was *practical*, and the minority report *theoretical*. In England, about 300 years ago, the Roman notation was considered *practical*, and the Arabic notation *theoretical*, and this identical distinction between *practice* and *theory* appears to prevail at the Franklin Institute to-day. The terms *practical* and *theoretical* are promiscuously used at the Institute, as a means of support to sciolism and perversion of the truth.

The difficulties which the French have experienced in establishing and introducing the metric system, are not tenable reasons for rejecting its adoption in this country. The difficulties Fulton had in introducing steam navigation, are to-day no objections to its use. The same can be said about Morse and the telegraph, and many other valuable advances upon which our progress and prosperity depend. The Republic of Switzerland and other nations who from French example have adopted the metric system, did not experience the difficulty with their *reamers* and *mandrils*, as intimated in the "practical" report.

The duty of technical and scientific men should be to consider, investigate and explain impartially, the comparative merit and demerit of the French and of our present system of metrology in all their bearings, and leave it for the law-makers to decide whether or not it would be expedient to introduce, or if necessary to enforce the metric system upon us. The majority of our committee, however, have taken it upon themselves to speak, not only for the Franklin Institute, but as though they represented the entire United States.

We have no substantial reasons for supposing that our law-makers would enforce unjust laws, and the Americans are generally a law-abiding people upon whom various laws are enforced every day. It is not for the Franklin Institute to decide whether or not the introduction of the metric system in this country would be an unjust law. We know from experience, history and tradition, that in all parts of the civilized world, communities do not always comprehend their true interests, and it has therefore been found necessary sometimes, to enforce laws by which to guide them into prosperity, as was the case in

England, with the introduction, adoption and enforcement of the Arabic figures for the Roman notation before mentioned. The enforcement of the Arabic figures in England, was made at the expense of burning the Houses of Parliament.*

In case our law-makers should find it expedient to introduce or enforce the metric system upon us, they will no doubt give at least ten years' notice, in which time the present *reamers* and *mandrils* in a tool shop may be worn out, and if not, they will not be likely to conflict with any clause in the new law.

The "practical" committee says, "the Franklin Institute has never "placed itself on record as opposing true progress." This statement conflicts with the tenor of their report, and moreover cannot be sustained in an impartial argument. The same committee is "favor-"able to the introduction of a perfect system of weights and meas-"ures," but they at the same time "hope that no such opportunity "may be presented in this country." If this paradoxical language is approved by the Franklin Institute, it may be interpreted and understood that this Society favors progress, but will not give any opportunity for it. I admit that to be true, because I have experienced the fact, but fear that such acknowledgment on the part of the committee would weaken the strength of their report. The same committee refers to an article published in the JOURNAL OF THE FRANKLIN INSTITUTE, headed, "The Metric System in our Workshops," which article contains the same kind of feeble ideas on weights and measures, as those in the "practical" report. The "practical" committee says: "The universe under this (metric) system, might be "compared to a great French clock, having the earth for its escape-"wheel, whose equatorial motion would be 400 metres per second." They evidently expect that such a "practical" idea is good enough to be approved by the Franklin Institute of the State of Pennsylvania, for the Promotion of the Mechanic Arts. The "practical" report is intrinsically imprudent, and, moreover, is ungrateful to the French Government and people, and if adopted as it now reads, it will *stamp a mark of old-fogyism* upon the Franklin Institute, which can never be wiped out, and under no consideration can that report accomplish the effect intended by its authors.

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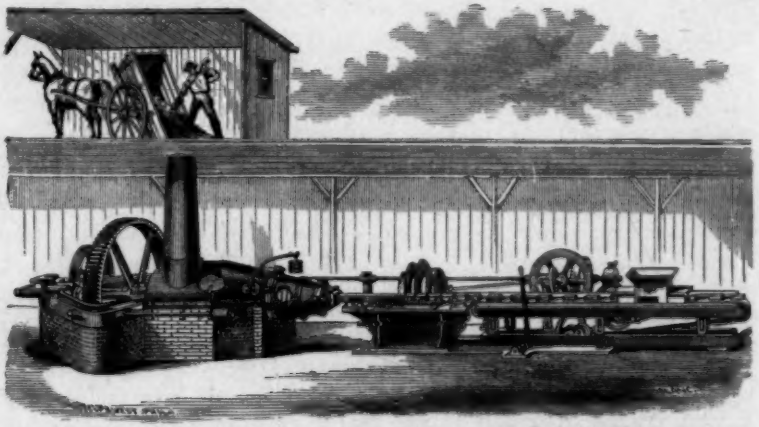
* It is a curious fact that the burning of St. Stephen's Parliament Houses was occasioned by the bon-fire of "tallies" of the old Exchequer accounts, which was made in a court yard.

I beg to be distinctly understood, that I do not advocate the introduction of the Metric System, nor am I against it or opposed to it; but only desire to see dispassionate justice done to it, and therefore feel it a duty to remonstrate against an unphilosophical and hasty disposition of so grave a subject, by a prejudiced Committee of our Society. The tenor of the "practical" report, moreover, seems to border so closely upon arrogance and partiality, as to be scarcely admissible by any institution of learning. A report of this kind ought to be devoted principally to substantial and essential facts bearing directly upon the expediency or inexpediency of introducing the metric system as the only legal standard of weights and measures in this country.

Description of Chambers, Brother, & Co.'s Archimedean Brick Machine, Philadelphia, Pa.*—This machine belongs to the class known as "*tempered-clay machines*," or "clay-tempering, brick-making machines," and was invented by the junior member of the firm, Cyrus Chambers, Jr., who is already well known to the public as a mechanic and inventor.



Chambers, Bro. & Co's Archimedean Brick Machine, Philadelphia, Pa.

The machine is constructed almost wholly of iron, and is made very strong and durable.

It *tempers its own clay with water*, taking the clay as it comes from the bank, without any *previous handling or preparation*, and forms it into bricks, with well-defined corners, and smooth, straight surfaces,

* Paper read before the meeting of the Franklin Institute, April 19, 1876.

at the rate of from fifty to eighty per minute, or from twenty-five to thirty-five thousand per day of ten hours.

The clay is taken direct from the bank and dumped at the side of a conical funnel that leads into the tempering case of the machine, and mixed, when necessary, with *loam, sand, or coal*, and the requisite amount of *water* being added to temper the clay to the proper consistency; the mass is shoveled into the hopper and falls into the machine.

The tempering portion of the machine consists of a strong iron case, in which revolves a horizontal shaft, into which are set spirally, strong tempering knives, or blades of steel, so that, as they pass through the clay, they move it forward. The clay being stiff, and not having much water on it, is not liable to *slip* before the knives, but is cut through and through, and *thoroughly* mixed; so that by the time it reaches the small end of the tempering case it is ready to be formed into bricks.

On the end of the tempering shaft is secured a conical screw, which revolves in a cast-iron conical case, the inside of which is ribbed, lengthwise, so as to prevent the clay from revolving in it, and is chilled, to prevent wearing.

The screw being *smooth* and very hard, the clay slides on the screw, thus becoming, as it were, a *nut;* the screw revolving, and not being allowed to move backward, the clay *must* go forward.

This operation further tempers the clay, and delivers it, in a solid, round column, to the *forming die*, which is of peculiar construction and form, and so designed as to reduce the round column to a rectangular one, whose breadth and thickness is the *proper breadth* and *thickness* for a brick, while at the same time it *forces the clay into the corners* of the square or rectangular, finishing part of the die, so that the angles of the bar of clay are made very solid and sharp, thus insuring perfectly square and well-defined corners to the bricks.

This bar, as it issues from the die, is conducted by a plate to the cutting device, which consists of a thin blade of steel, secured to the periphery of a wheel, in the form of a spiral, the distance between the blades of which is that required for the length of a brick.

This spiral knife runs perpendicularly, in an endless chain, which supports the bar of clay at one edge and bottom, so that the blade in passing through, the clay being supported, thus insuring the angles unbroken, and the cutting smooth and square.

The distance between the spiral blades being uniform, the lengths of the brick are absolutely uniform, thus overcoming the great practical objection hitherto existing in the Chambers' machine.

The drawing cut of the spiral, cuts the end of the bricks perfectly smooth, thus correcting another defect hitherto existing in this class of machines.

The speed of this spiral cutting blade is controlled by the clay itself; hence no matter how irregular the flow of clay, the spiral runs in exact unison therewith; consequently, the absolute uniformity in the length of the bricks.

This controlling of the speed of the spiral by the clay is so positive with that of the clay, that it will run at any speed, from one to one hundred bricks per minute, while the machine runs at full speed.

These cutting and regulating features are the new parts, and overcome the only valid objection heretofore urged against this machine as now introduced.

The bricks, thus cut from the continuous bar, are separated and carried by another endless belt through the dusting or sanding machine, which consists of a chamber, into which is thrown, by centrifugal force or a blast of air or steam, a continuous cloud of dust or fine sand, which adheres to the surface of the bricks, rendering them much nicer to handle, preventing them from sticking together on the barrows, or in the hacks on the *drying cars*, and much improving them in color when burnt.

PROVISION FOR STONES—All brick clays have more or less stones in them, and as it is impracticable to pick them *all* out, there is a necessity of making some provision for them, even if there should be only *one stone* in every "ten thousand of clay."

The tempering knives run six inches from the tempering case; hence there is no danger of a stone five inches in diameter catching between the end of the knife and the case, but they frequently imbed themselves in the clay that occupies the space between the ends of the knives and the case. If the stone is more than three inches in diameter, it will lodge at the entrance of the screw, preventing the clay from passing to it, and causing it to issue at the *safety valve*, through which the stone may readily be removed. If a stone, less than three inches in diameter, it will go through the screw, the openings between the threads being *less* at the entrance than at any other point, so that a stone that once fairly enters the screw, cannot lodge

until it has reached the forming die, where it will lodge, if it is *larger than a brick is thick*, and prevent the proper flow of clay, causing the bar to split in two, or only part of a bar to issue. This forming die is secured to the screw-case fly a *hinge* and *hook-bolts*, so that in less than one minute the die can be swung open and the stone knocked out, when the die is closed and the machine again started.

Small stones occur much more frequently, and pass freely through the machine, being buried in the bar of clay and passing to the "cut-off." When the stone should happen to be buried in the bar of clay at the line of severance, the knife must either cut through the stone, be broken, or some provision made by which it shall not be injured. The first is not practicable, as the stones are often very hard, and to break a knife every time it should happen to strike a stone would render the machine useless.

In order that the knife should not be affected by stones, the wheel to which it is secured is held in position by a spring which holds it with *just sufficient force to compel the knife to pass through the bar of clay*. When the knife comes in contact with any hard foreign substance, as stones, brickbats, or bones, the spring yields, and allows the knife to move back, and thus cuts to it, the knife immediately returning to its original position, ready to cut through the bar again—as the inventor demonstrated, by accidentally allowing his hand to remain in the cutting device when the knife came round, cutting two fingers "*to the bone*," when it yielded, leaving the bone uninjured, and the machine ready to cut the *next brick*.

This machine moulds the bricks stiff enough to be "hacked" at once in the drying sheds.

With these recent improvements, the inventor claims this machine to be the most practicable, durable, and economical brick-making machine yet introduced.

Improvement of the Passage over the Bar at the South Pass of the Mississippi.—The removal of the obstructions at the South Pass of the Mississippi, proceeding from the operations of Capt. Edes, is going on with unexpected rapidity. Already, a direct and navigable channel, from 200 to 400 feet in width, and 24 feet or more in depth, has been formed, reaching nearly two miles; and there remains less than 1000 feet yet in length to be excavated by the force of the current, where the channel is but partially formed, before the *jetty* pass will be open for the largest vessel. The action of the current, produced by the jetties on the bar, was somewhat singular.

The removal of earth has occurred on the up-river side, and has at once attained the extreme depth of 24 to 30 feet, which depth has gradually advanced in a straight line outwards towards the Gulf, until there remains only a crest on the very edge of deep waters, which is now (the last of April) 16 to 17 feet deep. At a short distance outside of the bar, there is a knoll of sand, upon which there was, according to the Coast Survey Reports of 1874, but 11 feet depth of water. This knoll has now (April) been washed away, until 15 feet in depth now exist; and, after this knoll is passed, there is found great depth of water in the Gulf. With the channel once established, years, almost centuries, will be gone before the sediment deposited beyond the present obstructions, will extend itself to form another delta, and the preservation of a permanent channel will depend solely on the maintenance of the heads of the jetties. The new pass has been in constant use for entering and departing vessels for six or seven weeks, and already is more direct and accessible than any other entrance to the river, bidding fair within a month's time, to allow the largest ocean steamer to enter therein. The restoration of the commercial importance of the City of New Orleans is confidently anticipated by its inhabitants as an immediate result from Capt. Edes' undertaking.

Bibliographical Notice.

THE NEW ENCYCLOPEDIA OF CHEMISTRY.—J. Lippincott and Co., Publishers, Philadelphia. 1876. Issued in 40 parts (10 already out), at 50 cents each.

This work is, in reality, a new and improved edition of Muspratt's "Chemistry as applied to the Arts and Manufactures," with many additions relative to more recent applications and improvements of practice during the past thirty years, completely revised in notation to correspond to the modern chemical nomenclature. Possessing all the excellence of arrangement of Muspratt's book, based upon the actual practice which he had so fully collected, the additions on the one hand, and the substitutions on the other, have been so well made, that the best compendium of applied chemistry in the English language has been prepared and offered to the readers of this encyclopedia. The complete work will be over 1000 pages of royal quarto size, in double column print, in clear and distinct typography, and upon the finest of paper. The profuse illustrations of the original work have been added too, and numerous steel plate engravings of apparatus in use, give a higher value to the whole for the practical man. No technical work of the order of merit of this one has before been offered at so low a price; and, as Muspratt's Chemistry already forms a part of the library of every manufacturer, this publication will now be found to be an indispensable substitute.

Civil and Mechanical Engineering.

DEEP-SEA TELEGRAPH CABLES: HOW THEY ARE TESTED.*

The "testing" of a telegraph cable, whether long or short, proceeds upon the principle that the materials offer to the electrical current a resistance: the testing of a cable is the measurement of this resistance. In any cable there are two kinds of these resistance measurements: one of the resistance which opposes the current in its progress along the conducting wire, the other of that which opposes its lateral dispersion. The conductor-resistance is technically termed the copper-resistance, and is extremely small compared with the other resistance. The lateral resistance to the escape of the current opposed by the insulating substance which surrounds the copper-conductor is technically termed the insulation-resistance. Where the resistance to the direct propagation of the electric current through a conducting wire is represented in units, the resistance to lateral dispersion through the insulator will be represented by hundreds, or even thousands of millions, of these units. A third property is that known as the electrostatic, or inductive capacity, or simply "charge" † of the cable; in other words, that measured quantity of electricity which the given cable will take up in a given time. So much for the necessary explanation of technical terms.

The copper-resistance (1), the insulation-resistance (2), and the "capacity" (3) are the three points to be ascertained in the testing of a cable; and it is useful to inquire why these are the points to be ascertained.

The chief commercial requisite in any cable, and upon which depends its value to its owners, is the speed with which signals can be transmitted. Speeds depends directly upon two of the foregoing points (that is upon the copper-resistance and "capacity"), and indirectly upon the insulation-resistance. Popular assumption is very much given to the idea that the electrical worth of a cable increases

* From *Nature*, London, April 27, 1876.

† "Capacity" and "charge" are not equivalent terms, although they are so considered in this article to prevent confusion, by the general reader, with the ordinary meaning of the word "capacity." The capacity of a cable remains constant, while the charge varies with the battery power employed.

with its insulation-resistance; as usual with popular notions this is only half-truth. That the cost of a cable follows the ratio may or may not be, but it is certain that above a definite limit the thickness of the insulating coating has no effect upon the practical working condition of the cable. It may be that minor indirect benefits arise, but with these, under the present consideration of the practical testing of a cable, we have nothing to do. A certain standard of insulation-resistance attained, there remain the two points, first, of the resistance offered by the copper-wire; secondly, of the charge. Now it is collaterally to be understood that, as there can be obtained through a pipe a greater flow of liquid when the pipe offers little resistance to the flow, so through the conductor of a cable can a greater flow be obtained when the conductor has low resistance. With most of the Atlantic cables, each nautical mile of the conductor has a resistance equal to that of three to four of the arbitrary units selected by the profession for comparison. There are in use two units of electrical resistance, namely, that determined by a committee of the British Association and the Siemens' unit. These units are very nearly of the same value, one Siemens' mercury unit (the resistance offered by a column of pure mercury of one metre length and one square millimetre section at 0° C.) being equal to 0·9536 of an Ohm,* the technical term for a British Association unit. There is, then, to be considered an electrical length as well as an absolute (or ordinary) length; the proportion that one bears to the other being known, the measures are convertible. Vague as may appear to the reader, this idea of electrical resistance; when he knows that two lengths of a copper wire of given diameter or weight, offer twice the resistance of one, he is as learned as the most skilled electrician, who virtually knows no more.

The consideration of the electrical capacity of a cable is more difficult. While the two other points relate to mass, the question of capacity involves that of surface, and of a property of the insulating material of the cable known as its "specific inductive capacity." The material with which long telegraph cables are insulated is gutta-percha. Two different cables may be insulated with this material to precisely the same dimensions, both as regards the thickness of the

*Many other standards of measurement of resistance were advocated, and the British association attempted to compare them, and in so doing, adopted a value (which was denominated an "Ohm," in honor of Prof. Ohm, of Nuremburg, who first propounded the theory of measurement of electrical resistance). The value of an "Ohm" is best understood from the comparison to a Siemens' unit, as in the text.

insulator and the thickness of the copper wire, but the "charge" taken by these cables may be very different, and the difference will be due to difference in the specific facilities offered by the two gutta-perchas to induction. This difference between various kinds of gutta-percha is as inherent as is the difference between resistances to conduction offered by different metallic alloys, and is probably very often due to want of homogeneity of the substance. It is by judicious selection and careful manipulation that the cable manufacturer is enabled to maintain a certain standard for any particular cable in question. Capacity, however, not only varies with the insulating material, but it also varies with the amount of surface of the conductor. It is different with different thicknesses of insulating material, but in this respect, after a certain limit has been passed, the decrease in capacity is very small for very large increase in the thickness of the insulating material.

High charge is incompatible with high speed. That cable will, other conditions the same, have the greatest speed in which the charge, or the fraction of the charge to be altered at each signal, is least. Professional necessity has given rise to a unit of quantity of electricity termed a "farad," of which the "micro-farad" is the millionth part. The capacity of a telegraph-cable generally ranges from three- to four-tenths of a micro-farad per nautical mile.

The object of testing a cable is, then, to ascertain whether the insulation reaches the amount specified, and whether the conductor-resistance and the charge are of the required minimum. As these tests are each applied separately to the cable, their consideration will fall under the several heads. It would clearly be impossible within the limits of this paper to describe the many methods which have from time to time been proposed and in use for the testing of telegraph cables. The first methods of testing submarine lines are undoubtedly due to Dr. Werner Siemens and Dr. C. William Siemens, who early in the history of submarine telegraphy communicated their researches on the subject to the British Association at the Oxford meeting of 1860. The principle of these early methods still remains the principle of the methods employed by Sir W. Thomson in his testing of the Direct United States Cable at Ballinskelligs Bay Station in September, 1875, and upon which he has reported to the manufacturers of the cable, Messrs. Siemens Brothers. It is the purport of this paper to describe these tests and the results obtained.

To those who may be unacquainted with the route of the Direct United States cable, it will be necessary to explain that the course taken is from Ballinskelligs Bay, on the west coast of Ireland, to Torbay, in Nova Scotia, whence it again passes to Rye Beach, in New Hampshire, America.

The construction of the cable, which was decided upon by the company acting under the advice of Dr. William Siemens, their scientific consultant, is as follows:—The cable from Ireland to Nova Scotia consists of a conductor formed of a strand of twelve copper wires weighing 400 lbs. per nautical mile. This conductor is surrounded with four coatings of gutta-percha and gutta-percha-compound weighing 360 lbs. per nautical mile, so that the total weight of the "core," as it is technically termed, is 760 lbs. per knot. It was specified that the core should have an insulation resistance per nautical mile equal to 160 millions of mercury units; tests, however, checked and taken under the direction of Mr. von Chauvin, the manager and electrician to the Company, show that no length of core was passed that did not insulate to nearly double this extent, or to 300 million units per knot, the tests being taken after twenty-four hours' immersion of the core in water at 75° F. The "core" is "served" or enveloped in jute yarn, and is then sheathed or covered with iron wires of a diameter best suited to the position of the cable. Thus for the deep-sea, 1,630 knots of the cable are sheathed with ten strands of wire and hemp, each strand consisting of a homogeneous iron wire surrounded with five strands of Manilla hemp, each strand being passed through a compound of pitch, tar, and india-rubber. Each of the iron wires has an average breaking strain of 53 tons per square inch, and is of 0·099 inch diameter. The cable termed medium cable is sheathed with fifteen wires of 0·148 inch diameter with proper sewings of yarn, while for the shore ends, where there is considered to be more friction or wear, this medium cable is again surrounded with iron sheathings of twelve strands of iron wires, each strand consisting of three iron wires of 0·230 of an inch diameter.

The cable from Nova Scotia to New Hampshire consists of a strand conductor of seven copper wires weighing 107 lbs. per knot, covered with three coatings of gutta-percha and compound weighing 150 lbs. per knot, and is also sheathed with iron wires.

The non-electrical reader who may choose to wade through detail that must be somewhat technical will perhaps find help in considering the conducting wire as representing a line of flow or force, such that

if two of these lines be directed into a galvanometer or current-measurer in opposite directions, that having the greatest head or greatest force will preponderate, while no indication will be found on the instrument when the forces are equal; also that from a known force giving through a known resistance a certain instrumental measure, any unknown resistance may be reduced when its instrumental measure is ascertained.

Testing the Resistance of the Copper Conductor.—Electrical measurements upon a long submerged cable differ from measurements made in the laboratory as described in text-books in one very important particular—that of earth-currents. Earth-currents are the *bête-noir* of the electrician, who not infrequently finds them so far masters of the field that his chance of obtaining accurate measures is a poor one. Fortunately, earth-currents do not have so much influence upon the working of a cable as they have upon the testing, and more fortunately still these currents do not always exist, so it is possible to obtain measures during a tranquil period. On the Direct United States Cable, Sir William Thomson found these currents to be equal in value at a period of greatest strength to that from about eighteen cells of the testing-battery—the Irish end being positive generally to the Nova Scotian end. Under such conditions, Sir W. Thomson employed the simple deflection-method of measuring the conductor-resistance, which he takes to be "the only proper method for measuring copper-resistance in a submerged cable." In the following description of the method and its results, it will be seen that the method consists in applying together with a measuring instrument an electric force which yields a certain measure through the unknown resistance of the cable; a known resistance (7300 units) is then substituted for the resistance of the cable, and the latter determined by proportion. The principle of this method is applicable not only to the measurement of the copper-resistance, but is that also of the ordinary method of measuring insulation-resistance, a higher known resistance being used in order more readily to effect comparison with the unknown and much greater insulation-resistance. The actual operation during the period of testing is thus described:—

"The insulation-galvanometer quickened three- or four-fold by a magnetic adjustment, and, with a shunt of twenty Siemens' units on its coil, was put in circuit between line, battery, and earth, and the deflection was observed and recorded every ten seconds. As was to

be expected, large and rapid variations of the deflections were continually taking place on account of earth-currents. The direction of the earth-current was from east to west the whole time, as was shown by the 'copper' current being always greater, and the 'zinc' current less, than the true mean concluded from the observations. It increased gradually (but with some slight backward pulsation) from the beginning—when its amount was that due to a difference of potentials between the Ballinskelligs and Torbay earths equal to 1·7 of a cell—till the end, when it was more than five times as strong, and corresponded to nine cells; the Irish earth positive relatively to the Nova Scotian earth the whole time. To measure the copper-resistance a time of comparative tranquillity was chosen, a reading taken, and then as quickly as possible the galvano-meter short-circuited, the battery reversed, the galvano-meter circuit reopened, and a fresh reading taken. Half the space traveled by the spot of light from the first reading to the second is taken, as being the deflection which would be produced by the battery applied in either direction were there no earth-currents. This was done seven times, and the half ranges were as follows:—235, 231, 229½, 234½, 231, 235, 230—mean 232·3. I found that the same battery applied in the two directions through the galvanometer, and 7,300 Siemens' units gave 232 divisions on one side of zero, and 233 on the other—mean 232·5. Hence the copper-resistance to be inferred from the observations is—

$$7300 \times \frac{232{\cdot}5}{232{\cdot}3}, \text{ or } 7306 \text{ Siemens' units."}$$

As the cable in question is 2,420 nautical miles in length, we have $\frac{7306}{2420} = 3{\cdot}02$ Siemens' units per knot.

Insulation Test.—The ordinary method of testing the insulation-resistance of a cable consists, as has been said, in obtaining upon the galvanometer or measuring instrument a certain measure with a known resistance, and a measure with the unknown resistance, the electric force being constant during the two measurements. From these two measures the unknown resistance is determined. If, for instance, it is known that with a certain battery power and a resistance of 100,000 units we have a deflection-measure of 100, it is deduced, when with an unknown resistance and the same battery power the deflection of 50 is obtained, that the resistance must be twice as great (namely 200,000), since the observed effect is halved. This system is

that generally pursued, but, like the other measurements upon submerged cables, comes under the effect of earth-currents; and to meet this contingency Sir William Thomson has arranged a new method, bearing upon the principle that the insulation of a cable may be determined from the proportion of loss (during a given time) of electric power that has been imparted to it. In the following description it will be seen that this loss is measured by the deflection due to the current entering the cable to make up the loss, and this deflection is compared with another deflection obtained by altering suddenly by a small quantity the battery power employed. The latter deflection being a measure of a known force or potential, the other measure for lost potential is determined, and consequently the loss of potential known.

"The cable being offered to me again from midnight till 2 A.M. on the 17th, I made," says Sir W. Thomson, "another series of tests at that time for the main object of measuring the insulation-resistance. I found the line in a much less disturbed state, and was able to make a perfectly satisfactory insulation test by the ordinary galvanometer method. I applied, however, also a new method, which (no electrometer being available) I had planned to meet the contingency of the line being disturbed by earth currents so much as to render the ordinary test unsatisfactory, but not so much as to vitiate an electrometer-test. This method, which I think may be found generally useful for testing submerged cables when an electrometer is not available, is as follows:—1. Apply the ordinary test by battery and galvanometer for a certain time. 2. Insulate the cable for a certain time and then shunt the galvanometer to prepare for No. 3 (unless you have conveniently available a second galvanometer suitable for discharges). 3. Instantaneously reapply the battery, through the insulation galvanometer properly shunted (or a special discharge galvanometer), to the cable, and observe the maximum of the sudden deflection produced. 4. Go on repeating Nos. 1, 2, and 3 as long as you think proper, according to circumstances. 5. To determine the proper ballistic constant of the galvanometer for utilizing the observed result of No. 3, find the maximum of the sudden deflection which takes place when a sudden change of electrification is produced by instantaneously changing by a small measured difference the potential of one electrode of the galvanometer, the other electrode being in connection with the cable. 6. The change of potential which, in the

operation of No. 5, would give the same deflection as that observed in No. 3, is equal to the change of potential which the conductor of the cable has experienced during the time when it was left insulated according to No. 2. Hence calculate the insulation-resistance in ohms or megohms* as in the ordinary electrometer method when the electrostatic capacity of the cable is known."

In carrying out this test, the 20-cell insulation battery (with its poles joined through 20,000 Siemens' units) was applied, zinc to cable, through the insulation galvanometer with a shunt of 5,000 Siemens' units on it. Then, the galvanometer indication was read and recorded every ten seconds for three and a half minutes, when the cable was insulated during a minute according to No. 2 of the directions above, and a shunt of 30 substituted for the 5,000. At the end of the minute the battery was instantaneously reapplied, the throw of the galvanometer observed according to No. 3 and the shunt of 30 removed, and 5,000 reapplied. The cable was again insulated for a minute, the galvanometer shunted with 50 (instead of 30 used the first time), and the operation of No. 3 repeated. The proper ballistic constant of the galvanometer was determined by applying alternately full power and $\frac{1}{20}$ of full power of the insulation battery; the change from one power to the other being made in each case as instantaneously as possible. Twelve galvanometer readings taken at ten seconds' intervals during the second and third minutes of the electrification gave for mean deflection 127, and the readings taken from the fourth to the twenty-fourth minutes gave for mean deflection 82·1. The sensibility of the galvanometer in the condition in which it was used for these readings was such that a deflection of 290 would have been given by the actual battery, with a resistance of 1,000,000 Siemens' units. Hence the insulation-resistances proved by the mean observed deflections were for the deflection, 127 from the second and third minutes 2,280,000 Siemens units, and for the mean deflection 82·1 from the fourth to twenty-fourth minutes 3,540,000 units. The new method described gave, as the mean of the observed ballistic deflections or "throws," the number 89·8, or say 90. The ballistic deflection due to instantaneously changing the potential by $\frac{1}{40}$ of that of the insulation battery, in accordance with the rule of one to five above, was found to be 112 divisions. This is $1\frac{1}{4}$ times the preceding mean throw (90), which therefore showed a change of potential equal to $\frac{1}{50}$ of that of the

* A megohm equals a million ohms.

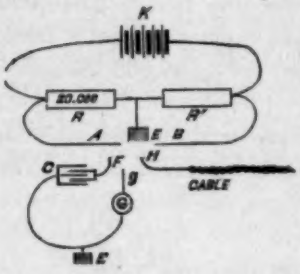
battery. Hence the mean fall of potential was $\frac{1}{40}$ during the minute, or at the rate of $\frac{1}{3000}$ per second. The capacity of the cable (measured in the way presently described) had been found to be 991 microfarads. Hence the insulation-resistance is $\frac{3000}{991}$, or 3·027 megohms, or 3,170,000 Siemens' units, corresponding to the 3,540,000 units given by the ordinary method. With copper to line, a fresh series of tests gave 3,520,000 megohms, or 3,690,000 Siemens' units.

In the reduction of the insulation-resistance of the whole cable to its insulation-resistance per knot, it has to be observed that as the insulation of the cable is inversely as its length, one knot of the cable will give an insulation resistance equal to that of the whole cable multiplied by the number of knots' length in the whole cable.

Measurement of "*Capacity.*"—Just as the chemist has his vessels for measuring out quantities of liquid, so has the electrician his special arrangements for measuring out quantities of electricity; but there the analogy ends, for while the measure of the liquid is direct and visible, the electrician infers his measured quantity generally by the mechanical work effected on the index of the measuring instrument, or by the absence of such work. The apparatus used in practice for measuring quantities of electricity is termed a "condenser." "Condensers" are constructed having any required capacity, and if such a condenser of which the capacity is known is charged from a battery, then discharged through the measuring instrument, and the deflection produced noted, it is only required to charge from the same battery the cable or any other condenser of which the capacity is to be measured, then to note this discharge deflection, and by proportion to deduce the unknown capacity. On short lenghts of cable this procedure is actually adopted, but on long lengths it becomes liable to error, chiefly from the fact that as with long lengths some perceptible time is required to discharge the cable, the ballistic throw or sudden deflection produced upon the measuring instrument by the rush of electricity from the cable does not measure all that passes out. It is consequently necessary to devise some method like the following used by Sir W. Thomson, in which the charge from the cable (communicated thereto by a different battery power to that charging the condenser, but the relative powers being known) is neutralized by a charge of opposite electricity from the condenser, and the neutralization declared by the non-production of movement in the measuring instrument.

The following diagram, which is not, however, taken from the report, will explain the method:—

K, battery of 80 cells, well insulated; B, resistance of 20,000 units; B′, variable resistance; C, condenser of 80 microfarads' capacity; G, shunted galvanometer; E, earth.



The condenser is electrified by bringing F and A into contact, and the cable by making contact between H and B, for sufficiently long time to fully charge the cable. These contacts are then broken, and instantly after, contact made between F and H. This contact is maintained for five to ten seconds, when the additional contact with *g* is made. The variable resistance is adjusted till this last contact produces no movement on the measuring instrument.

It was found that when the cable and condenser were charged to opposite potentials in the proportion of 1,615 to 20,000 no throw occurred, whence the deduction that the capacity of the cable was

$$\frac{20000}{1615} \times 80 \text{ microfarads, or}$$

991 microfarads, and the length of the cable being 2,420 knots, this was equal to 0·409 of a microfarad per knot.

In concluding the report upon the electrical conditions of the Direct United States Cable, Sir William Thomson remarks: "I am glad to be able to say that my test proved the cable to be in perfect condition as to insulation, and showed its electrostatic capacity and copper resistance to be so small as to give it a power of transmitting messages, which, for a transatlantic cable of so great a length, is a very remarkable as well as valuable achievement." This article would be exceeding its purpose if it were to include inquiry into the present position of Atlantic Telegraphy: but it is a mark of great progress in electrical engineering and cable manufacture that a cable of such length as 2,420 miles can be delivered up to the company working it in a *perfect* electrical condition. This has not been the case in earlier transatlantic attempts; and some idea may be formed by the general reader of the care required to bring about this end, when it is known that a small hole, smaller in size than the finest pin-hole, in any portion of the two thousand miles length of gutta-percha covering would render the electrical conditions of the cable imperfect.

Climbing a Standpipe.—Some reparations having become necessary upon the standpipe at Spring Garden Station of the Philadelphia Water Works, amongst which the scraping and painting of the exterior, which had become weather worn and rusted, was the most considerable task, the first step to be taken was obviously to construct a scaffold for the workmen; and, as no means had been provided for the attachment at the top of the pipe of the blocks and falls from which a scaffold should be suspended, the climbing of the pipe for this purpose was an undertaking which preceded all others.

This climbing was accomplished by Mr. George Robinson (a working rigger of this city) in the following way:—

The standpipe itself is 127 feet of wrought iron shaft, above a square stone plinth, the shaft being about 6 feet in diameter at the bottom, and 4½ feet in diameter at the top (under the cap, or head, ornament, which projects 12 or 16 inches all round).

At the foot of the plinth, a light ladder, 30 feet long, was set up, with the top to rest against the shaft. Climbing the ladder to the top, carrying a bow or ring of ½ inch round iron rod, which was made to surround the shaft loosely, with the ends about 16 inches long, turned downwards, these ends were lashed fast to each side of the ladder. Next, a piece of rope (3 inch = 1 inch diameter), with an eye in one end, was passed also around the shaft, and was lifted to the top of the ladder, below the ring of iron, when the plain end of the rope was drawn through the eye and made fast, so that the rope formed a "lashing," and encompassed the shaft tightly. A single block was now hooked on to this lashing, and the end of the fall passed down between the ladder and the shaft, was made fast to the lower round of the ladder, and the ladder itself then hauled up to the lashing; and with its upper end steadied by the ring of iron, was placed vertically against the side of the shaft. Another ring of ½ inch iron was placed around the shaft at the bottom of the ladder, which ring was also lashed to the sides of the ladder, and steadied the bottom whenever it was attempted to lift by the lower round. The ladder being elevated as described, and held in place by making the hauling side of the fall fast to something below, another lashing like the first one, was taken to the top of the ladder (in point of fact, Robinson stood upon the top of the ladder each time it was hauled up, and took with him this second rope); and this rope was then converted into a second lashing like the first one, only about 25 feet higher up on the shaft.

A second block was hooked into this second lashing, and the end of a fall from it was taken down behind the ladder to the lower round, and made fast, while the other end was hauled tight to relieve fall number one. Lashing number one was now cast off, and taken to the top of the ladder, and by means of the second fall the ladder, with Robinson upon it, was lifted to the second lashing. At this point, the operation merely repeated itself, except that, from the reduced diameter of the shaft, it was necessary to bring the head of the ladder up to the lashing and make new ends, to the top bow of iron (which could be bent cold) twice in the whole climbing. The bottom ring it was not found necessary to reduce in dimension. Five fleats brought Robinson to the top of the shaft, and as the top of the ladder was then hung far enough from it, he was able to pass at once over the projection of the cap, and mount upon the plates which covered this projection (a low ornamental railing surrounds the cap). Having reached the top, the other attachments became easy. The man Robinson, and another rigger to handle the rope, aided by one or two men, when a pull was required, performed alone all the labors of the task. They came to the Spring Garden Works at about 10 A.M.; and in less than two hours (before 12 M.) the column had been climbed, and the ladder was sent down.

Chemical Nomenclature.—A remarkable paper upon this subject by Prof. Henry Wurtz, appears in the March number of the *American Chemist.* Prof. Wurtz enunciates a theory of molecular volumes which he applies to numerous examples, and exhibits the results. The basis of the theory is, first, that there is a constant temperature of nature, which is normal for all substances, and is about —1° centigrade, and at this temperature all molecules will have diameters, which will refer to a molecular diameter for ice of 2·7, expressed in even tenths for multiples. While it cannot be said that Prof. Wurtz's results possess all the simplicity of expression or application which might be desired, yet the direction of the inquiry is highly instructive; and without expecting the even numbers as a result, it is very possible that some *law* of variation of temperatures for various chemical bodies, may allow definite relative volumes to be established with as much certainty as the atomic combinations.

Note.—A very valuable article upon the Determination of Strains in Continuous Girders, by Prof. A. Jay DuBois, is unavoidably deferred until the July number of the JOURNAL.

Errata.—Please correct in article of Prof. Du Bois on Roof Strains, page 243, lines 14-15:—For 525, read 461; and for 437·5, read 341.

Chemistry, Physics, Technology, etc.

"THE PHYSICS OF THE ETHER."*

By William B. Taylor, Washington, D.C.

Bold speculation, based upon judicious induction, and conscientiously brought to the earliest available test of phenomenal accordance, is undoubtedly the means, and the occasion of scientific advancement. If ever the great mystery of force, or of matter, is to be unveiled, it will be by this process; and the very frequent, though somewhat supercilious disdain of speculation or "hypothesis"—whether by the positivist on the one side, or the teleologist on the other,—is neither based on the caution of scientific fealty, nor justified by the teachings of intelligent experience. The human tendency to form guesses, however, is unfortunately vastly in excess of the tendency to make careful comparisons of observation. The first is exceedingly easy,—the latter, often tedious and laborious, requiring patient vigilance and generally skilled training. And a necessary result of the rapid prevalence of the scientific method over the metaphysical in modern times, has been to relegate to a lower and less disciplined order of intellect, the crude and unfashioned vagaries which two or three centuries ago so prominently characterized the leaders of "philosophy."

The work before us, extending to 136 octavo pages, is written with much diffuseness, and with frequently recurring repetitions of statement; as though its 25 chapters or "sections" had been produced at irregular intervals, and had been collected from various sources of publication. Its author belongs to a not inconsiderable class of students and thinkers, whose activity of imagination, unbalanced by an equal inertia of solid logic, leads them to find in "motion" a sufficient explanation of all the phenomena of *attraction* or *repulsion*. Like others of his class he is puzzled by—and cannot "comprehend" action at a distance; therefore he is clear that it must be a fallacy.

* Physics of the Ether. By S. Tolver Preston. London, E. & F. N. Spon, 1875.

The modern dynamical theory of gases,* in which the temperature represents simply the amount of molecular motion at any moment existing in the given volume, is too enticing an apparent analogy, not to have been very often pressed as a type of all energy. And hence the inter-stellar medium or vehicle of radiant light and heat, is made to do duty for a vast number of very incongruous effects.

"As a first condition, we require the existence of a physical agent capable of transmitting motions to a distance with speed and facility, in order to refer to physical agency all the effects at present brought under the theory of 'action at a distance.' As a second condition, this physical agent must be shown to be capable of enclosing a store of motion of a very intense character, competent to produce all those forcible molecular and other movements of matter, exhibited in such effects as the phenomena of chemical action, the 'electric' motions, combustion, the remarkable development of molecular motion observed in the case of explosive compounds, such as in the explosion of gunpowder, and other numerous and striking phases of motion witnessed on all sides. Thirdly, this physical agent will have to be proved to be competent to exert an intense pressure upon the molecules of matter, as consonant with the very forcible character of the static effects exhibited in 'cohesion,' and the stable aggregation of molecules generally." (*Physics of the Ether*, *p* 10.)

Such is the programme offered, as the alchemy of the universal solvent of *force*.

"The quality of elasticity in an aeriform medium, by which it can expand and fill a larger portion of space than it occupies under normal conditions, and in which act motion is developed, must be dependent on a motion previously existing. Hence since it is an observed fact that the ether possesses in its normal state the quality of elasticity, the inference necessarily follows that the normal state of the ether particles must be a state of motion," (p. 12). "To summarize therefore: the inferences are, first, that the normal state of the component particles of the ether is a state of motion; second,

* "The greatest achievement yet made in molecular theory of the properties of matter, is the kinetic theory of gases, shadowed forth by Lucretius, definitely stated by Daniel Bernoulli, largely developed by Herapath, made a reality by Joule, and worked out to its present advanced state by Clausius and Maxwell." Sir Wm. Thomson's Address to the British Association in 1871. (*Report B.A.*, vol. xli, p. 98.)

that this motion of the particles takes place in straight lines; and third, that this motion takes place towards every possible direction," (p. 14).

But here a very formidable difficulty is at once encountered. How is this inevitable "elasticity" of ether to be evolved from "motion"? Two colliding atoms of the ether, are either deformed, or not deformed by their contact. If not deformed, there can of course be no recoil; and there is simply the loss of the entire *vis viva*. If deformed, they either continue so, without further effect, or they regain their original form with a resilient force which is ordinarily designated as "elasticity." What is this resilient force? Alas, the keenest human wit has hitherto been able to give no other answer, than to accept the observed fact as an *ultimate* one, and to call it by its most obvious character, "repulsion." The conception is of course not essentially changed, if we suppose the atom perfectly hard, and the resilience exerted by an aura of repulsion.*

Here then at the very outset of the purely *rational* system of universal kinematics, the disciple of Motion is brought face to face with the dreaded phantom of an occult property,—the "incomprehensible" *static* force—"repulsion." Nor is there any escape, however resolutely the disciple may close his eyes to the catastrophe. Without the conserving and restoring spring of elasticity, the machinery of force (as well as of motion) is forever at a stand-still.

Mr. Preston first attacks the problem of cohesion.

"Steel of the best quality, in the form of fine wire, has been known to bear a tensile strain represented by not less than 150 tons per square inch, and even this cannot be said to be the limit to the tensile strength of steel, since the tenacity obtainable increases as the diameter of the wire is reduced," (p. 16).

But must not the bond of "chemical affinity" be sometimes even stronger than this?

"If we suppose the intensity of the pressure to be overcome in the case of the separation of the molecules of oxygen and hydrogen

* An ingenious attempt has been made to explain elasticity kinetically by assuming that the ultimate atoms, instead of being deflected by collision, revolve in orbits of extreme eccentricity, whose outward excursions represent the repellant force. But 1st, such revolving atoms must transmit their entire centrifugal force at the first impact with external particles, remaining inert forever afterwards; and 2d, their original orbital motions could have been attained only by the coercion of an *attractive* force.

one of the most powerful cases of chemical union, to be only three times greater than that overcome by the separation of the molecules in the example given of the steel wire, then this would give 450 tons per square inch as the difference of pressure effective in the case of the molecules of oxygen and hydrogen. This is, however, not the total or normal ether pressure, but only the effective *difference* of pressure; however, as our object is only to arrive at a limiting estimate for this pressure, or to fix upon the lowest value consistent with what observed physical facts would require, we will accordingly take in round numbers, the estimate of 500 tons per square inch, as a limiting value for the ether pressure; having thus valid grounds for concluding that this estimate is within the facts as they actually exist," (p. 18).

Now we have what may be called the modulus of mobility of the ether, expressed in the velocity of light,—185 thousand miles per second. "The normal velocity of the ether particles must at least equal that with which they can transmit a wave;" and cannot probably be many times in excess of this. Mr. Preston's method of arriving at the *density* of the ether, is as follows: Supposing that the average velocity of aerial particles at ordinary temperature be taken at 1,600 feet per second, producing a dynamic pressure of 15 pounds per square inch, then since dynamic pressures are as the velocities squared, the number of times that the square velocity of air (in feet per second) is contained in the square velocity of luminiferous ether (in feet per second) may be taken as the number of times the mass or density of air exceeds that of ether. This would be about 390 thousand million times, (p. 20).* This, however, is

* Sir John Herschel, from the squares of the velocities of light and of sound, has estimated that the elastic force of ether "in proportion to the inertia of its molecules" is about one billion times that of air; and accordingly to compress the ether to a density equal to that of ordinary air (at 15 pounds to the square inch) would require a pressure of some 15 billion pounds (6,700 million tons) to the square inch. *Familiar Lectures*, 1866. Lect. vii, "On Light," sect. 65.) But as the velocity of wave transmission is not only as the square root of the elasticity of the medium directly, but *inversely* as the square root of its density, it is evident that the above estimate, based exclusively on elasticity, must be very much too high: for if the rarity of the ether were a million times greater than that of air, then this alone would give a thousand fold greater velocity to light than sound, leaving but a million fold excess of elasticity.

Mazotti has computed that a density of ether, equal to one 360,000 millionth of

giving the ether only the same dynamic pressure as the atmosphere. But the pressure previously assumed for the ether, is 500 *tons* per square inch, which is 74,666 times the ordinary atmospheric pressure; and hence the ether must be this many times *denser* than the above estimate; or its rarity must be reduced to only a little over 5 million times the rarity of ordinary air, (p. 20).

Now these *data* being admitted, how is the dynamic pressure of an ether so constituted, supposed to act, in order to hold so firmly together the molecules of steel, while having free access to all sides of each molecule? Mr. Preston here builds entirely on the sandy foundation of Mr. Guthrie's experiments with vibrating tuning-forks, detailed in the Philosophical Magazine for Nov., 1870, (vol. xl., p. 345,) in which suspended pieces of paper, and other light objects, were observed to approach the vibrating body; and with a rate proportioned to the amplitude or energy of the vibration. Counting from the absolute zero, Mr. Preston estimates that the thermal energy of vibration of the molecules of steel, at 60° F., is equivalent to that of a fall through about 7½ miles, corresponding to a velocity of 1,600 feet per second, (p. 31). The vibrating molecule then is the "motor" which is to simulate Mr. Guthrie's tuning-fork in producing approach by another body.

"If the question be fairly examined into, there exists no other conceivable process by which one mass or molecule of matter could move or act upon another mass or molecule placed at a distance, than by means of vibration. For in the first place, in order for a mass of matter to be capable of moving or physically affecting a

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that of mean or ordinary air, would account for the observed acceleration or diminution of period of Encke's comet. (Mrs. Somerville's *Mechanism of the Heavens*, book iii, chap. 6.) Mr. Harkness of the Naval Observatory at Washington, however, estimates that the resistance of Encke's comet would be explained by a rarity of ether, exceeding Mazotti's result by about 144,000 times; or about 52,000 billion times the rarity of the ordinary atmosphere. (*Washington Observations*. Report of Mr. Wm. Harkness on Encke's comet in 1871.)

It is proper to add, however, that Encke's hypothesis of a *resisting* medium in space (so generally accepted) is by no means established, or even rendered probable, by the isolated case of a single cometary retardation: which, as Mr. Asaph Hall, of the Washington Naval Observatory, has well shown, is susceptible of quite a different interpretation. (*Am. Journal of Science*, 1871, vol. ii, p. 404.)

second mass placed at a distance, without approaching it, the mass must have a motion of some kind, so as to be capable of disturbing the surrounding medium which forms the only physical connection between the masses; secondly, since the mass or molecule in acting upon the second molecule maintains a fixed position, it follows that the motion of the molecule must take place in such a way that the molecule can maintain a fixed position, and nevertheless can disturb the surrounding medium. Now a vibratory motion of the molecule constitutes the only conceivable means of satisfying these conditions, as by this form of motion, the molecule can retain a fixed position by oscillating about a fixed point, and yet can disturb the surrounding medium by its motion. Hence in a mechanical point of view, nothing can be more obvious or to the purpose than the vibratory motion of matter so constantly presenting itself in physical phenomena," (p. 33).

Surely,—" nothing can be more obvious!" But a little difficulty again suggests itself.

" A molecule of matter surrounded by the ether cannot possibly be in motion without disturbing the ether, and thereby giving up or dissipating continually its motion in the surrounding ether," (p. 33).

The tuning-fork expending its motion on the resisting air in order to produce a mechanical effect on the suspended card, could not continue its vibrations a single minute, did not Mr. Guthrie stand by with his violin-bow to excite the motion. But where is the " prime motor " to fiddle the molecule into continued activity?

" Since, therefore, the motion of matter is being continually dissipated in the ether, the ether constitutes the *receptacle* of all the motions of matter. The ether, therefore, must in accordance with the principle of conservation, be the *source* of all the motions of matter; for matter cannot evolve motion out of itself. Also, since matter cannot retain its motion, but must be always dependent on the ether for any supply of motion, matter therefore cannot in any case constitute a *source* of motion. The ether therefore constitutes both the source and the receptacle of all the motions of matter," (p. 90).

Here then in this vicious " cyclical process " of perpetual motion, we have the notable secret of cohesive attraction. The all-moving ether stimulates the molecules of matter into an activity, which, re-

acting upon the ether, produces therein waves of fresh energy so exuberant, that the ever-dancing molecules are kinetically bound together in bonds of—literal *steel*! And thus blindly pursuing the analogy of gaseous thermo-dynamics, as the writer fondly imagines, he reaches his goal—with the *dynamics* quite omitted. According to the doctrine of correlation and conservation of force, as based on the inductions of experimental observation, the continuity of thermal impulse on the moving particles of a gaseous medium, is maintained *only* by the fresh impacts resulting from the recoils of minute successive "falls" of material molecules: and to the extent that these "falls" are completed or terminated, must the "working" power of the derivative gaseous motion rapidly decline. By Mr. Preston's pan-kinetic scheme, there is no such action as a fall in nature; there is only propulsion; the propeller being always ultimately the thing propelled.

After this general exposition of the "cyclical process," it might seem hardly necessary to follow in detail the method by which this wonderful bond of vibration is supposed to be effected.

"If we suppose the imaginary case of a vibrating molecule of matter completely isolated, then stationary vibrations could not be formed in the medium, the equilibrium of pressure of the medium about the molecule could not be disturbed, and the molecule would be in equilibrium. But if we suppose a second molecule is placed in proximity, then the medium about the molecule is specially disturbed at that side where the second molecule is situated, due to the formation of stationary vibrations in the medium intervening between the molecules by mutual reflection of the waves; a rarefaction of the intercepted vibrating column of the medium, which abuts against the opposite molecules, being the result. The condition of equilibrium of the molecules will therefore depend upon the fact whether the pressure of the intervening vibrating ether column is greater than, equal to, or less than the ether pressure at the remote sides of the molecules, where the normal ether pressure exists; the molecules being driven in the one direction or in the opposite, or remaining in equilibrium, according to the relation of these two pressures," (p. 44.)

"When the molecules are placed in very close proximity, the intercepted ether column becomes short relatively to its breadth; the lateral area afforded for expansion becomes contracted, and the pul-

sations of the column are more concentrated against the opposing molecules, tending to separate them; also the energy of the stationary vibration of the column has been increased, owing to the increased proximity to the molecules. . . . . On the other hand, when the separating distance of two molecules is increased, the intercepted ether column becomes long relatively to its breadth, more lateral area is afforded for expansion, which would conduce to a rarefaction of the column," (p. 45).

After a suitable amount of expansion of this idea, the author concludes:

"Since a rarefaction of the medium constitutes the only possible physical means by which an 'attraction' (approach) can be produced by vibration, and further, since a rarefaction is only possible under the condition of a stationary vibration of the medium, it therefore follows that a stationary vibration can be the sole physical cause concerned in producing an 'attraction,'" (p. 49), *Q. E. D.*

The same mechanical agency which is thus supposed to be adapted to control material particles within infinitesimal distances, and efficient to hold them there in a stationary equilibrium of enormous tension, is also proposed for the very different and incongruous duty of controlling the same particles at unmeasured distances, in a movable equilibrium, and by a bond which is at once the most instantaneous (if the degree may be allowed) and incomparably the feeblest of all known forces. Newton has shown that two spheres each of one foot diameter, and having the mean specific gravity of the earth (5·5)—"if distant but by one-fourth of an inch, would not even in spaces void of resistance, come together by the force of their mutual attraction, in less than a month's time." (*System of the World.*)

No explanation is attempted of the peculiar nature of etherial vibrations, which, able to maintain a constant differential impulse upon every molecule of two such masses for 30 days, without producing in either ball a motion of more than one-eighth of an inch during all that time, are yet (with the speed of light) infinitely too slow to represent the action between the sun and the earth. We are simply told that

"The vibrations of masses or molecules of matter are in all cases necessarily attended by a rarefaction or displacement of the intervening medium: this conclusion holding, however distant the masses may be from each other, or however feeble the vibrations," (p. 43).

This conclusion, however, is justified by no known laws of physics or of hydrodynamics: and if it were, would still prove utterly inadequate to illustrate the subject. And this crude application of a mistaken dynamic analogy, capable of expressing no one ascertained characteristic of gravitative action, is gravely propounded as a full and exhaustive exposition of celestial mechanics. We are asked to believe in a system of "stationary vibrations" (with an infinity of nodal points) extending through more than 90 million miles, or in the case of the planet Neptune, though considerably beyond 2,000 million miles, though with a constantly changing radius vector, from perihelion to aphelion; while as an etherial vibration requires about four hours to reach Neptune, it is evidently impossible for the line of "impulsion" to be rectilinear. And we are asked to believe that these vibrations will produce a line of "rarefaction" (straight or curved) while the mobile ether is supposed to exert on all sides of this extended line, a dynamic pressure of 500 tons per square inch. What effect on the vibratory column of rarefaction between two distant bodies, the sudden interposition of a third body will have, as in the case of transits and eclipses, we are not informed.

(To be continued.)

ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE LAST TEN YEARS.*

By Dr. A. W. Hofmann.

From the *Chemical News*.

[Continued from Vol. ci, page 356.]

Being cooled to the temperature of spring water it passes into an expansion piston machine, where it yields power in the same manner as steam. In consequence of the great expansion there is a considerable fall of temperature (see below), sometimes reaching —20° to —30°. The gas as it escapes from the machine can be used for

* "Berichte über die Entwickelung der Chemischen Industrie Während des Letzten Jahrzehends."

cooling any article, for producing ice, and, finally, it may serve for the manufacture of soda water. The machine shown at the Exhibition was two horse power. The cost of the gas is said to be quite covered by the value of the copperas produced; 1 cwt. sulphuric acid (costing 5 florins Austrian) and 1 cwt. iron spar (1 florin 50 kreutzers, together 6 florins 50 kreutzers) yielding 240 lbs. copperas (at 3 florins per cwt.), worth 7 florins 20 kreutzers. The idea of this combination is doubtless ingenious, and in individual cases it may prove remunerative in practice, but for general application it is not suitable. One cwt. iron spar yields about 20 kilos. carbonic acid, which, at a pressure of 5 atmospheres, represent 2 cubic metres. On expansion to 1 atmosphere, this quantity can theoretically produce at most a working power of 170,000 kilogrammetres, equal to 1 horse power for about half an hour. By expansion, the gas loses about 200 heat units, and at the outside the same amount is available for the production of ice, yielding not more than 2 kilos. Hence it is plain that exceedingly large quantities of materials are requisite, and that it would be difficult to find a sufficient demand for the copperas. The escaping carbonic acid, also, can be but very partially utilized in the manufacture of soda water, and would require for this purpose to be compressed anew, so that it would appear more advantageous to pass it at once into the water as it issues from the generator.

If the carbonic acid is used for making soda water as it issues from the ice receiver, the absorption would take place at about —5°, under a pressure of 33 atmospheres, or about six times more than is necessary. The power which the machine must exert to produce this superfluous pressure is totally wasted. The latent vapor heat of carbonic acid is certainly not yet known, but even if it were high (which, from the great specific gravity, is improbable), enormous quantities of carbonic acid would be required to produce only a moderate yield of ice, far larger than there is any prospect of utilizing in the manufacture of effervescing beverages. We can, therefore, prognosticate no success for the above mentioned apparatus, and consider that the use of carbonic acid in a circulatory process would be more rational.

The Ammonia Machine.

At common temperatures ammonia is a gas; under pressure it may be condensed to a liquid. The temperature and pressure of liquid ammonia, accord ing to Regnault, are respectively as follows:

Temp.—	+40°	+20°	0°	—20°	—30°	—40° C.
Pressure.—	15·5	8·5	4·4	1·84	1·16	0·7 atmos.

Ammoniacal gas is readily soluble in water, which at 0° it saturates with 1050 volumes, or 0·875 in weight. At 20° water absorbs 654 vols., or 0·52 of weight, a little more than half its own weight. In this state it forms the liquid ammonia of commerce. The absorption is attended with a considerable rise of temperature, whence the latent heat of ammoniacal gas may be calculated as 500°, or close upon that of steam. The gas absorbed by water can be entirely expelled by the application of heat. A decrease of pressure has the same effect, in which case the temperature falls.

If aqueous ammonia is heated in a closed boiler the expulsion of the gas can be carried on even under a strong pressure. If the gas liberated is conducted into a cooler at a certain temperature it passes the point of saturation and condenses to form liquid ammonia. This liquid anhydrous ammonia if brought in connection with a receiver containing water, rushes into it with violence and is absorbed. In proportion as the solution is heated, the temperature of the evaporating liquid is lowered, and may fall to —50°. On these principles depends the ingenious apparatus constructed by Ferd. Carré. It has two modifications, adapted respectively for the intermittent and for continuous production of ice, the former on a small scale for quantities of 1 and 2 kilos., the latter for manufacturing purposes, and arranged so as to turn out 25 to 200 kilos. hourly. The apparatus is manufactured by Mignon and Rouart, of Paris. The intermittent machine consists simply of two vessels free from air and connected firmly by means of a tube. The weight of the whole is such that it can be conveniently lifted and turned. One of the receivers contains ordinary liquid ammonia, whilst the other is empty. The operation begins by placing the receiver containing ammonia over a charcoal fire, whilst the empty one is set in a tub of cold water. The gas is expelled by the heat and condensed in the cold receiver, forming a liquid. When all the ammonia has passed over, the apparatus is taken up, the receiver which had previously been heated is placed in the tub of cold water, and the substance to be frozen is put in a tin cylinder, fitting into a concavity of the receiver containing the liquefied gas. The latter evaporates, producing a great reduction of temperature, and is again absorbed by the water which has remained in the other receiver.

The continuous apparatus is more complicated. It consists princi-

pally of a vertical cylindrical boiler supported by masonry in which the heating and volatilization of the ammonia for the preparation of liquefied ammonia goes on without interruption. The cylinder consists of two compartments, the lower containing a very dilute solution chiefly deprived of its ammonia, whilst in the upper are a number of bowls, into the uppermost of which flows fresh liquor ammoniæ; the liquid as it overflows enters the next bowl, and so onward. The lower compartment only is exposed to the fire. The very aqueous vapors given off convey as they ascend more and more ammonia into the bowls, which at last evaporates almost, though not absolutely, free from water. The gas now arrives in the cooling apparatus, in which it is condensed to a liquid. The pressure at which this takes place depends on the temperature of the condenser, and varies from 4·4 to 8·5 atmospheres, when the temperature of the water used for cooling ranges from 0° to 20°. The temperature of distillation is about 130°. From here the liquefied ammonia, at the pressure of the boiler, arrives in a regular current in the evaporator (ice-generator), the influx being checked by a regulator. The arrangement of the ice-generator presents nothing worthy of remark. A resolution of chloride of calcium takes up the cold and transfers it to the water to be frozen. If a liquid, *e.g.*, beer-wort, requires merely to be cooled, no intermediate body is requisite. The ammonia evaporating in the cooler has to be absorbed by water. As an absorbent is employed, the liquid in the lower half of the cylinder, which is not quite exhausted and which issues continuously in a thin stream, arrives cooled in the absorption vessel. The latter, again, must now lie in a cooler in order that the high temperature produced by absorption may not hinder the further reception of gas. Thus, therefore, liquor ammoniæ is formed as at first, and is thrown back into the cylinder by means of a pump, exchanging on the way its heat for that of the liquid drawn off from the lower compartment of the cylinder. As the ammonia evaporating in the cylinder is not quite anhydrous, a certain amount of water reaches the ice-generator, where it gradually accumulates and retards evaporation. From time to time, therefore, the contents of the cooler must be drawn off, and pumped direct into the cylinder.

The first intelligence concerning the apparatus here described is contained in a communication by Carré to the Paris Academy, December, 1860. * The inventor's English patent bears the date October 15,

* Carré, *Comptes Rendus*, li, 1023.

1860. Not long after in January, 1861, MM.* Tellier, Budin, and Hausmann, sen., claimed priority in the invention, which they had patented in July, 1860.† For machines on the small scale they recommended at the same time sulphurous acid instead of ammonia, since, though less soluble in water, it requires only half the pressure. The courts of law appear, however, not to have decided the question of priority in favor of these gentlemen, as their name does not occur in connection with the further development of the machine. We find, however, that Tellier, in the year 1862,‡ issued a report in which he recommended ethylamin and methylamin for use on the large scale instead of ammonia. The vapor of the latter is absorbed by water in double the volume of ammonia and possesses a very slight tension, so that the internal pressure in the apparatus scarcely exceeds one atmosphere. Hitherto, however, we do not learn that machines for the application of these substances have come into use. Since Tellier has recently—as we have mentioned above—constructed an air-pump machine for methylic ether, it may be suspected that peculiar difficulties have been found in the utilization of these amines. A detailed description of Carré's continuous machine with illustrations has been published by Pouillet.‖ An illustrated account of the small intermittent apparatus may be found in the *Wurtemberg Gewerbeblatt* for 1861, No. 40, and has been copied into other journals.§ In 1868, the author drew up for the *Badener Gewerbezeitung*, a paper on ice machines, based upon experiments undertaken with the small machines then known.

In this essay Carré's small apparatus is described and illustrated by diagrams. The larger, or 2 kilos. size, yielded $2\frac{3}{8}$ kilos. ice with a consumption of $\frac{3}{4}$ kilo. of wood charcoal, the time of heating being 80 minutes, and that of freezing two hours. Ingenious and effective as is this apparatus, it cannot be recommended for household use, as its manipulation requires too much technical skill. In the *Badener Gewerbezeitung* for 1869, followed an account of the machines for manufacturing purposes, in which Carré's large machine is described

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* Tellier, Budin, &c., *Comptes Rendus*, lii, 142.

† *Dingler, Polyt. Journ.*, clx, 23 and 120.

‡ Tellier, *Comptes Rendus*, liv, 1188. *Dingl. Polyt. Journ.* clxv, 450.

‖ Pouillet, *Bull. Soc. d'Encouragement*, 1863, 32. *Ding. Polyt. Journ.* clxviii, 171.

§ *Ding. Polyt. Journ.*, clxiii, 182.

and figured. It was then and there announced that two German firms, Kropff and Co., of Nordhausen (since 1867), and Vaas and Littmann, of Halle on the Saale (since 1868), have taken up the manufacture of ammonia ice machines on Carré's principle. The former of these firms has now become a joint stock Ice Machine Company. According to the most recent quotations, both these establishments furnish the larger apparatus in five sizes, yielding from 25 to 500 kilos. of ice per hour at the price of 4800 to 30,000 reichsmarks (say from £240 to £1500). The Nordhausen Company manufacture also a small apparatus, for 7½ kilos. per hour, for 2250 marks (£112 10s.). According to their statements 1 kilo. of coal, according to the size of the machine, produces from 6 to 16 kilos. of ice.

The author in his treatise gives a calculation which verifies these statements. In the above-mentioned experiments with the hand machine 1 kilo. of charcoal yielded 3½ kilos. of ice. Vaass and Littmann give estimates of the first cost of the various machines, and of the price of the ice produced, which varies according to the size of the apparatus, producing respectively from ½ to 10 cwts. hourly, from 1 mark 15 pfennige to 20 pfennige per cwt., including interest on the capital, depreciation, and waste. The Nordhausen Company gives a calculation for a 250 kilos. machine based on 300 days' uninterrupted work (day and night), according to which the ice costs 36 pfennige per cwt. Up to the end of the year 1873 the latter establishment had finished 60 machines, 29 of them for Germany. Vaass and Littmann had completed 42 machines, 20 of which were for Germany, including 2 for Vienna.

At the London Exhibition of 1862, and that of Paris, 1867, Carré's machines were exhibited by Mignon and Rouart, of Paris; at the Vienna Exhibition the two German firms made their appearance.

Carré's machine is without doubt a very perfect, manageable, and effective apparatus for producing ice everywhere and to any extent. In many cases it may successfully compete with the natural article, especially in large towns where the demand is great, and where luxury plays an important part. Artificial ice is often more palatable, since natural ice is too fequently dirty, and even when perfectly clean possesses a swampy flavor. Among the ice manufactories whose existence we have ascertained, may be mentioned that of A. Pokorny, in Vienna, which was supplied by Kropff, in 1869, with a machine yielding 5 cwts. hourly. The proprietor has courteously informed

the author that he is perfectly satisfied with the working of the machine. It yields 10 cwt. ice per 1 cwt. of charcoal consumed. The cost of the ice amounts to 35 Austrain kreutzers per cwt., the sale price fluctuating from 70 kreutzers to 3 florins 20 kreutzers. The loss of ammonia amounts to 2 kilos. per 50 cwts. of ice produced. The machine has remained in good order for four years, but the connections are not perfect. Ice machines have been set up in various German breweries, to which the makers refer in their circulars. Mignon and Rouart are said to have made in 1869 a machine of the value of 20,000 florins (?) for the Joint Stock Brewery, at Deux-ponts.*

Carré's machine has been from its very origin a very carefully constructed apparatus in which essential improvements are scarcely conceivable. The difficulty as regards the material was soon overcome by making all the parts of wrought-iron coated with zinc, copper, and its alloys being entirely avoided, as they are rapidly attacked by ammonia. Reece, however, in 1870, patented in England an improvement with the object of preventing the simultaneous evaporation of the water in the boiler. He asserts that the liquid which arrives in the ice generator consists of 25 per cent. of water and 75 of ammonia. His arrangement, which corresponds in the main with dephlegmation and rectification as customary in distilling, is said to condense the ammonia practically free from water.† He also utilizes the tension of the evaporating ammonia in a machine which works the pumps.

In September, 1867, Toselli, of Paris, obtained an English patent (in the name of Clark) for an ammonia ice machine arranged on the principle of Carré's portable apparatus. It consists of two cylinders, united axially by means of a tube, and turned continually by a handle. The ammonia contained in one of the cylinders gave off, when heated, its ammoniacal gas into the other cylinder ; the residual water afterwards re-absorbed the gas, the apparatus remaining all along hermetically closed. It was planned both for domestic use on the small scale and for manufacturing purposes. As regards its performance nothing has transpired.

Ammonia Air Pump Machine.—In 1869, Mort and Nicolle, of

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* *Dingl. Pol. Journ.*, cxciii, 432.

† Reece, *Dingl. Pol. Journ.*, 1870, 40. See also *Chemical News*, vol. xxxiii, p. 130.

Sydney, patented an ammonia machine* differently arranged from that of Carré's and capable of being regarded as a combination of the latter with the ether machine. The inventors use an air pump, but assist its action by absorption. As cooling agent they apply not volatilized ammonia, but concentrated aqueous ammonia.

The ammonia remains dissolved in the water only under the pressure at which it was saturated, and escapes in proportion as such pressure is diminished ; at the same time the liquid is cooled in a corresponding degree, as during the evaporation of pure liquid ammonia. Mort and Nicolle produce this decrease of pressure by means of the air-pump. The ammonia removed is condensed by the return stroke of the piston, and along with a corresponding amount of the diluted liquid simultaneously withdrawn from the evaporator is forced through a cooler, where re-absorption takes place. This arrangement requires less motive power than a pure mechanical condensation. It is to be expected that this ammonia machine should be more efficient than the ether machine, but its performance falls short of that of Carré's machine. More exact accounts are hitherto not to be had. The machine works at lower pressure than the ether machine, and like this must be carefully protected against the influx of air. The danger of explosion is removed from the machine itself and transferred to the boiler of the engine.

About the end of 1870 Mort and Nicolle patented a new ammonia machine, of which the only description in our hands is the English specification. It is described as the "low-pressure ice machine," and agrees in principle with Carré's machine, the air-pump being omitted. It differs, however, from the latter machine in as far as not liquid anhydrous ammonia, but a highly concentrated aqueous ammonia is produced and evaporated. This of course requires a much lower boiler pressure, the maximum tension being about 2 atmospheres at a steam heat of 107° C. The evaporation of course produces a much smaller reduction of temperature. The arrangement is such that the ammoniacal liquid steaming from above the ice generator, and flowing slowly over horizontal depressions, gradually loses its ammonia, and arriving at the bottom in a very reduced state of concentration is drawn out by means of a pump, and in a especial vessel re-absorbs the ammonia which has been expelled by the heat. The liquid restored to its original degree of concentration is

* Mort and Nicolle, *Mech. Mag.*, 1870, 189. *Dingl. Pol. Journ.*, cxcvii, 311.

pumped back into the ice generator. The liquid escaping below from the kettle, and which is little more than water, serves as in Carré's machine for the re-absorption of the ammonia evaporating from the ice generator, and is driven back into the boiler by a second pump. It must be mentioned as a specialty that the ammonia evaporating in the boiler passes first into a cylinder with a piston, and furnishes the power for working the pumps, whereupon the absorption takes place.

(To be continued.)

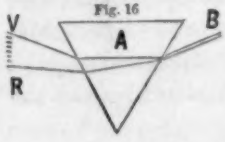
A LECTURE ON LENSES.

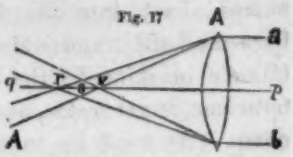
By Joseph Zentmayer.

(Continued from Vol. ci, p. 347.

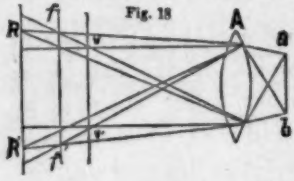
So far we have considered a ray of light, refracted by a transparent medium, to be still a single ray. Such would be the case were the white ray of light of a single homogeneous color; but what we call white light is composed of different colored rays, which by pass ing through a refracting medium, are refracted in different degrees. This is the source of another aberration of even more importance than the spherical aberration—the chromatic aberration. By passing a beam of white light, *B*, (Fig. 16) through a prism, it is not only refracted, but decomposed into seven colors, red, orange, yellow, green, blue, indigo and violet. These different colored rays are differently refracted by the prism. The violet ray, as the most refrangible one, is refracted towards *V*, and the red one, as the least refrangible, is refracted towards *R*, and other colored rays fill out the space between *V* and *R* in the order of their refrangibility. This is known as dispersion. The dispersion of refracting medias is measured by the length of the spectrum which they produce. Flint glass has more dispersive power than crown glass, because the spectrum which it produces is longer than that of crown glass. The dispersion of a medium is indicated by the difference of refraction between the index of refraction of the red and the violet. Let us now see what effect the dispersion has on images produced by single lenses.

Fig. 16





White light *a* and *b* is falling on a double convex lens (Fig. 17). The ray *a* is decomposed into the different colored rays as soon as it enters the lens, and the red ray, as the least refracted, will cross the axis *p q* in *r*, while the violet ray crosses the axis in *v*. Between the red and violet the other colored rays cross the axis. The same is with the ray *b*, and if we do not consider the spherical aberration of the rays between *a* and *b*, all the red rays will have their focus at *r*, and all the violet ones at *v*. Between *r* and *v*, the foci of all the other colored rays are situated. The space between *r* and *v* is called the longitudinal, chromatic aberration. The length of the aberration changes with the dispersive power of the media, out of which the lens is made; it is, for instance, twice as great if the lens is made of flint glass, as if the lens were made of crown glass. The influence of the chromatic aberration on the image of a lens is shown in Fig. 18.



The white light from the object *a b*, refracted and dispersed by the lens *A*, does not form a colorless image at *f f'*, but the red rays form one at *R* and *R'*, and the violet at *V V'*. But between there, an endless number of colored images of rays of different refrangibility are produced. The red image is the largest. If we place a screen at *R R'* we do not get simply a red image, as all the other dispersed images are formed on the screen; and as the mixing of all the different colors of the solar light make white light again, so the mixed images, that is, the central part is colorless and only the margin is blue, because it is surrounded by the diffusion image of the blue diverging rays.

If the screen is moved to *V V'*, then the image is surrounded by a red margin; if it is moved to *f f'*, the colored margin disappears, but the image, composed of the different dispersed images, appears undefined and not clear. This effect is more increased because each colored image has its spherical aberration also. Chromatic aberration alone would place the different colored images in regular succession behind each other, but spherical aberration mixes these images of different colors, and only the two outer ones, red and violet, remain. From the foregoing it is clear that chromatic aberration must necessarily interfere with the definition of a lens, and that it is desirable

to find a way to correct this evil. From the moment when Newton unraveled the nature of solar light, proving that light is composed of rays of different refrangibility, our greatest philosophers and opticians have spent their time and skill in the attempt to produce lenses without chromatic aberration, or at least to reduce it to a minimum. Sir Isaac Newton was of the opinion that refraction and dispersion of different refracting substances are always in the same ratio to each other, and concluded, that it was hopeless to produce refraction without color, by combining convex and concave glasses. Leonhard Euler, the great mathematician, on the other hand, reasoned in another way, and this is a curious instance of how a correct conclusion was drawn from false premises. He assumed that the human eye is achromatic, and consequently a lens could be made achromatic too, and Newton must be in error; he constructed theoretical rules for making achromatic lenses, and Dollond, the optician, succeeded in carrying them out. But Dollond, by comparing the eye with his lenses, observed that the eye cannot be achromatic, and Fraunhofer afterwards measured the chromatic aberration of the human eye, and found that an eye, that is able to bring parallel rays of red light to focus on the retina, can only bring violet rays to a focus, coming from a distance of two feet.

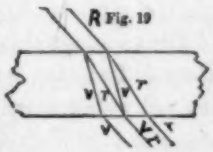


Fig. 19

Now let us see how we get rid of these beautiful colors, which we admire so much in the rainbow and the glittering dewdrop, but which hurts the eye of an optician, in an optical instrument. If a ray of white light, *R* (Fig. 19), falls obliquely on a parallel plane glass, it is decomposed as soon as it enters the glass; but on the other side all the colored rays which made white light, are, on leaving the glass, parallel to its former direction, and if we think the whole surface of the glass struck by white rays, they all will be dispersed, and come out parallel on the other side; but, if the different colored rays are mixed homogeneously, it makes white light again.

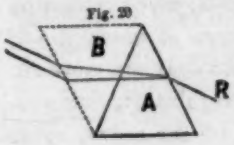


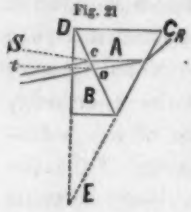
Fig. 20

But if a prism, *A* (Fig. 20), is struck by an oblique ray, *R*, the ray is dispersed in the glass, and the colored rays leave the prism diverging, and they cannot be properly mixed again to white light, except we can give to the leaving rays their parallelism again. Now, if we combine a prism,

B, of the same angle and material, in a reversed position to *A*, it is evident that we restore the diverging rays again to parallel rays; but, unfortunately, we destroy not only the dispersion, but also the refraction,—we make a thick, parallel glass out of the prism.

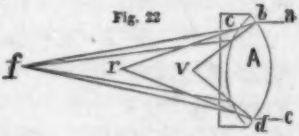
Let us try it in another way. The ray, *R* (Fig. 21), passes into a prism of crown glass, *A*, and a colored image would be formed at *s t*, if the prism *B* would not interfere. If we now could combine with the prism *A*, one of a less angle, but made from material like flint glass, of greater dispersive power, so as to have the same dispersive power as the larger angle prism *A*—we can restore the diverging into parallel rays, and the light will come out white again, although it went through the compound prism *C D E*. This is perfectly practicable, if we make the prism *B* of flint glass; this having a greater dispersive power than the crown glass, and the rays *c* and *d*, when entering the prism *B*, are somewhat refracted—the violet more than the red—and their divergency is smaller; and, if the prisms have the right proportions, the red and violet rays come out into the air parallel, and, at the same time, the rays passing from the prism *B* will have a different angular direction than that with which they entered the prism *A*. Thus we have refraction without dispersion.

Fig. 21



Let us adapt this principle to a lens, *A* (Fig. 22), made of crown glass. The rays *a* and *c* enter the lens at *b* and *d*, and are dispersed; the red would cross the axis at *r*, and the violet at *v*. We associate the plano concave *c*, of flint glass, with the lens *A*. As the negative flint lens is of a denser medium, the violet, as well as the red rays, will be refracted, but the violet more so than the red; and, if form and dispersive power of the two lenses are in the right proportions, the red, as well as the violet, will meet at the point *f;* the image formed there, is colorless, or achromatic, or, in other words, it will appear in its natural colors. But even in the best achromatic lenses there is still a small amount of color left, which cannot be destroyed. If we compare the spectrum of a prism of crown glass, with one of flint glass of the same angle, we find that the more refrangible blue, indigo and violet, take not only absolutely, but also relatively, more space than one in the spectrum of the crown glass

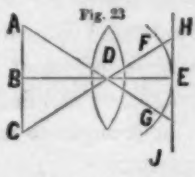
Fig. 22



prism. So, if we succeed in uniting the outer rays, red and violet, the intermediate colors cannot unite completely, and this remainder of not corrected colored rays we call the secondary spectrum. Complete achromatism, therefore, cannot be obtained, but we must be content to come as near as possible to the requirements. A selection of crown and flint glass, in which the proportion of length of the spectra of the different rays are nearly related, will bring us very near to our purpose. Fortunately, the colors of the secondary spectrum are feeble, and do not interfere much with the sharpness of the image, and we are well pleased if a lens exhibits only the secondary colors—light purple and greenish, as it is a proof that the most objectionable effects from chromatism are removed. The association of flint and crown glass serves not only to correct chromatic aberration, but, as we have seen before, if the right form for each of a pair of lenses be selected, it corrects spherical aberration also. Such a lens, corrected for spherical and chromatic aberration, we call an *aplanatic* lens.

We now come to another aberration of a lens, the curvature of field. The image of a flat object, formed by a lens, cannot be received on a plane screen; the screen ought to be concave. *A*, *B*, and *C* (Fig. 23), are very distant points, and, therefore, nearly equal distant from the lens *D*, of which the point *B* is situated, in the line of the axis of the lens, while the points *A* and *C* are above and below the axis. It is evident that the images of these points are formed at nearly equal distances from the optical centre, not far from the principal focus. The field *FEG* is therefore curved, and cannot be received on the screen *HI* equally sharp. The curvature of field is generally attributed to spherical aberration; sometimes it is even thought to be spherical aberration itself, but it has nothing to do with it. If lenses could be made with parabolic curves, free of spherical aberration, the curvature of field would be about the same.

Fig. 23



Suppose we have a globular lens *A* (Fig. 24), with a diaphragm in the middle, so small as to reduce spherical aberration to almost nothing. Now we know that the focus of a sphere of crown glass is situated $\frac{1}{4}$ of the diameter behind the globe, at *B*, and as all the pencils are normal, they all will form their image $\frac{1}{4}$

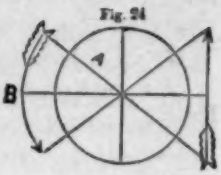


Fig. 24

of the diameter of the globe behind it; that is, the image lies in a curve, concentric with the lens, although the spherical aberration is not perceptible. To understand the correction of the curvature of field, we must make clear what is meant by depth of focus, and what the effect of a diaphragm is. Depth of focus is the property of a lens, to give a tolerably clear image of objects, not in one plane. Figs. 25 and 26 will make it plain. In Fig. 25, we make use of the whole aperture of a lens *D*; *R R* are parallel rays, striking the margin of the lens. The image is formed at a screen *A*; if the screen is moved to *B* or *C*, the image of the point *a* spreads out, because the angle of the crossing rays is large. When the same lens, *D* (Fig. 26), is provided with a diaphragm, so as to reduce the aperture considerably, the focus of the rays *R R* is still at *a*. If we now move the screen the same distance as before, to *C* or *D*, we find that the image of the point *a* is considerably reduced. If we now look at Fig. 23, we see that only *E* can be sharp on the screen, and if the screen be moved towards the lens until the points *F* and *G* are sharply defined upon it, then the point *E* will lie beyond the screen and become indistinct; but if we provide the lens with a small central diaphragm, we can find a place for the screen, where all three points can be brought to it, without the images being sensibly diminished in sharpness.

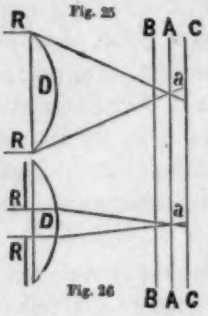


Fig. 25 Fig. 26

Now let us see what takes place, if we move the diaphragm to a proper distance from the lens. *A*, *B*, *C* (Fig. 27), are distant points, *L* a converging lens. Let us trace the course of the rays, commencing from the points *A*, *B*, *C*. The rays from he points *B*, situated in the axis, and the image of the point *B* will be formed at *F*, the principal focus. But it is different with the rays coming from *A* and *C*. The rays proceeding from the point *A*—A_{I}, A_{II}, A_{III}, A_{IV}, A_{V}, are refracted to *a*, *b*, *c*, *d*, *e*; similarly the rays from the point *C* are refracted to *a′*, *b′*, *c′*, *d′*, *e′*; occasioning, as we have seen

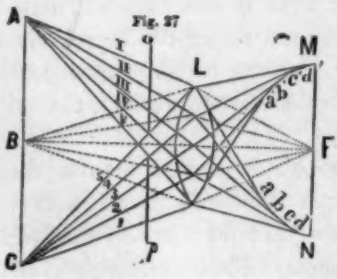
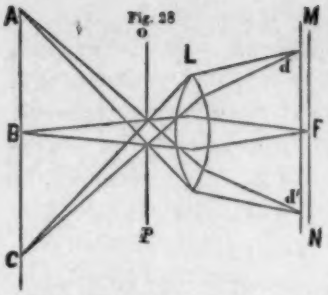
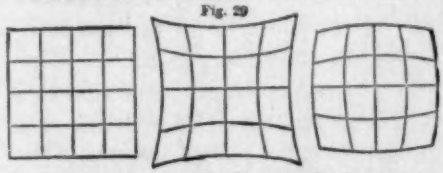


Fig. 27



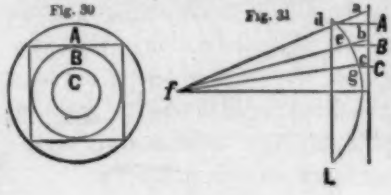
before, spherical aberration. If we place a screen at the principal focus *F*, it will not receive a distinct image, even if we have a concave screen; as will be observed, all the rays outside of the axis arrive at different distances behind the lens. You notice that none but the rays A_1, and A_2, and C_4 and C_5, have their focus near the plane of the screen *M N*. Now if we find a place for a diaphragm, so that only these rays pass the lens, and the depth of the lens is as great as *d M*, we may expect a pretty sharp image on a plane screen. By looking over the figure, we see that such a plane is in *O P* (Figs. 27 and 28). A diaphragm in this place, and of the proper size, will allow only the most favorable rays to pass, and a tolerably flat and sharp image is obtained. The smaller the diaphragm, the sharper and flatter the image. But as we mentioned before, small diaphragms have the disadvantage that the light is cut off to such an extent; and for most purposes the lens becomes useless. But suppose we would employ a negative lens, under the same conditions, we would have no real image, but a virtual one, the curvature of the field would be reversed, and the marginal rays have a longer focus than the central ones. Therefore, it is possible to associate a negative with a positive lens, and to render the field flat.

The next aberration which we have to deal with, is the distortion.



If we describe a network of straight lines, and hold a convex lens over it, placing the eye at a distance from it in the axis of the lens, only the two right angle lines of the centre appear straight; the others appear curved. When the upper is in the reverse position to the lower one, they appear pincushion shaped. When distortion of the negative lens is reversed, the lines appear as the curved sides of a barrel.

The cause of distortion is somewhat difficult to explain, but the following figures make it clear. Let us describe upon a plate or plane surface a number of circles, *A*, *B*, *C*, equidistant from each other (Fig. 30 in front view, and Fig. 31

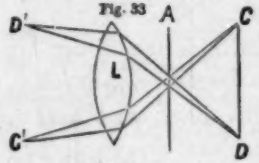


in profile), and place in front of them (Fig. 31) the lens *L*. Now the rays which proceed from *A*, *B*, *C*, parallel to the axis of the lens, strike it at *d*, *e*, *g*, from whence they will be refracted, and meet at *f*, the principal focus. If we place the eye at *f*, we see the circle *A*, not where it really is, but in the direction *f d*, the circle *B* in the direction *f e*, and the circle *C* in the direction *f g*. By prolonging the lines of directions, until they meet the plane of the circles *A B C*, we observe that the circles do not appear equally apart, but their distance is increasing from *C* to *A*; they will appear as in Fig. 32.

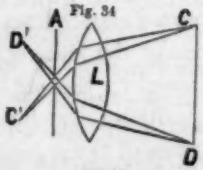
We will suppose for a moment that the circles *A* and *B*, (Fig. 30),

are of such relative diameters, that a square inclosing *B*, with its sides tangential shall have its corners in the circle *A*. Now if we draw the circle *A* and *B*, (Fig. 32), (as they will *appear* from *f*), the distance between *A* and *B*, will be greater, or equal to *a b*, (Fig. 31), and as the contact of the side of the square with *B*, (tangentially), and with *A* at the ends must be kept, the line of the side will now *appear* curved or bent (Fig. 32).

A single lens without distortion cannot be made, but by combining two or more lenses in connection with diaphragms in a certain position, the distortion may be corrected completely. If a diaphragm is placed in front of a lens *L* (Fig. 33), different parts of the lens are



employed to form different parts of the object *C D*. In this case the distortion is barrel shaped, but by placing the diaphragm behind the lens, as in Fig. 34, the distortion is of the opposite nature, that is, pincushion shaped. Rays coming from *D* (Fig. 33), pass through the upper part of the lens, while in the latter, through the lower part.



Now you will readily see, that by uniting two lenses, equal to each other, *L*, *L'* (Fig. 35), and placing a diaphragm *D* between them, it follows that the distortion accompanying the lens *L*, with its diaphragm behind it, is corrected by the action of the same diaphragm, upon the rays entering the lens *L'*, where the diaphragm is now in front of the lens *L'*. The modern photographic objectives to be used for architectural work and copying, are constructed on this principle.

Unfortunately this advantage is obtained at the sacrifice of aperture, that is, of light. I mentioned before, that the negative lens has the opposite distortion of the positive lens, so that by proper combination of lenses of suitable curves and material, distortion can nearly be overcome upon a limited field.

Photographic objectives used for portrait purposes, when a large quantity of light is desirable for brief exposure, are thus corrected; but these are again open to the fault of a restricted angle of vision. In all other lenses when the light is the desirable element to be preserved, the correction of distortion must be made, as far as possible, by a combination of lenses.

We come now to the last of the more important aberrations, that is the astigmation, a word coming from the Greek, meaning: not coming to one point. If we focus a well defined, round object, situated in the axis of a lens of a wide aperture, on a screen, we find the image round, even if we move the screen in and out of the focus, the image will get only less sharp; but if we turn the lens sideways, so as to get the image of the same object formed by pencils oblique to the axis, then we will observe that it is no longer possible to form a sharp image of the objeet, and by moving the screen in and out of the focus, the image appears elongated, horizontally or vertically.

Now let us see whether it can be made clear, in the following figure (36). *C D* is a convex lens, of which *A B* is the axis. The lens is represented in perspective, as we have to show two planes, in different directions. The radiating point *R* is situated at infinity, and outside of the principal axis. We will lay a plane through the axis *A B*, and the point *R*, which will cut the lens in its diameter *C D*. Let us lay another plane through the point *R*, at a right angle to the former, and which will cut the lens in its diameter *E F*. If we draw the line *R p* through the optical centre of the lens, a ray following it would not be refracted, as we have seen before, and constitutes a secondary axis. *R p* is the line where the two planes cut each other, and consequently belongs to both planes Let us draw the two extreme rays, *R' C* and *R' D*, of the diameter *C D*, which, after refraction are *T* and *p'*, as we learned by analyzing spherical aberration. If we now look to the other plane, the rays

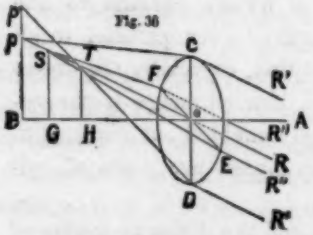


Fig. 36

R'' F and *R''E* are symmetrical to the axis, and are exactly equally refracted, meeting at the point *S*. If the lens is now diaphragmed down, so as to improve the aberration of the plane *CD*, we find that we have for one lens two distinct foci. If we focus, for instance, a brick wall, we will have the horizontal white mortar lines in focus, while the vertical ones are out of focus, and *vice versa*. By looking to the figure, you can easily see that that universal doctor in optics, the diaphragm, will also cure astigmation, at least will bring it to a minimum. Fig. 35 will suggest a way by which astigmation may be destroyed almost completely. The diaphragm *D* divides the lens *L* into an infinite number of lenses, of which each acts on a different radiating point, and the pencils in or out of the axis, strike the lenses almost normal, hence such a combination is not only nearly free of distortion, but of astigmation also.

Many of you are aware that in nearly all human eyes there exists an aberration, also called astigmation. Although in its effect similar to the astigmation of lenses, just mentioned, it is of a different character. Nature intends that the curves of the cornea and crystalline lens of the human eye should be spherical; but the exceptions seem to be the rule. The curves of the cornea and crystalline lens of the eye are in nearly all cases, more or less elliptical, egg-shaped, and consequently have in one meridian a longer focus than in the other. If such an eye brings the image of a line parallel to one meridian, to a focus at the retina, the images of lines parallel to all the other meridians, do not collect at the retina, especially the one at right angles to the former, and a distorted, blurred image is the result. The advancement of science has lately enabled our oculists to correct this evil by spectacles, of which the glasses are parts of cylinders, instead of spheres.

Now, knowing all the defects of lenses, and the different modes of correcting the same, let us look back to that primitive instrument—the pin-hole camera. The pin-hole camera is free from all the errors, as spherical and chromatic aberrations, distortion, curvature of field, astigmation; and the only objection against it is the extremely small aperture. What an amount of speculation and hard labor of the most eminent men were necessary to furnish a substitute, equally free from errors, having a larger aperture, giving a brighter image. And, even now, none of the aberrations can be completely corrected, and the best that can be done, and that for a limited aperture only, is to

reduce the errors so far as to diminish their extension, so as to make them appear to our eye at a smaller angle than the eye is able to distinguish. In lenses used as objectives, where the image is magnified by high eye pieces, even that is extremely difficult, as the errors are also magnified. Our most celebrated opticians, such as Fraunhofer, never attempted to give a telescope objective a larger aperture than the focus divided by ten, except in very small pocket telescopes. And his larger telescope, the one he made for the Dorpat Observatory, and which he considered his best objective, has a focus of 160 inches, while the aperture is only 108 lines, that is 1-17th of the focal length, and its highest magnification is 720 times. The larger telescopes of Dollond are nearly twice as long. The same artist, Fraunhofer, took precaution to warn young opticians and amateurs not to listen to the very natural desire to try their skill on larger apertures, and giving higher magnification, if they do not wish to be disappointed, and lose time and money. But the school of experience seems to be the only one to cure this desire.

But here I feel bound to mention that a few years ago, Mr. Steinheil, of Munich, read a paper before the Academy of Sciences of that city, on an improved telescope objective. It is composed of four lenses—one positive crown glass lens, combined with a compound negative lens, which itself is a triplet of two flint and one crown glass lenses. By this formula, a 4-inch telescope is only two feet long, while in the ordinary way it is twice as long.

While I am speaking about wide apertures, I cannot pass without mentioning a very serious obstacle connected with large apertures; it might be called the parallactic error. I was frequently asked why a large photographic objective does not give the same sharp image that a small one does. It is somewhat more difficult to correct a large objective than a small one, even if the aperture stands in the same relation to the focal length. But it is not only this. Suppose we have a large photographic objective, say of six inches aperture, *L* (Fig. 37). Each part of the lens receives radiating rays from each point of the object, and brings them to a focus at the respective place. Now, if we cover the lens by pasting paper over it, leaving only the aperture *A* free, we still get an image, only more feeble in light. Again, cover the aperture *A*, and open the aperture *B*, you get an image of the same object; but the apertures *A* and *B* are say four inches apart.

Fig. 37

Both cannot give precisely the same image, as they are taken from another base. The images will be similar to the two images of a stereograph, which are taken in a similar way by two lenses. Now open both apertures, *A* and *B*, and as the images are not equal, they cannot cover each other, but will overlap, especially the images of the nearer objects. If we now use the whole aperture of six inches diameter, it is clear that we will have an infinite number of images none equal to the other; every one overlapping the other, and the image, necessarily, must be a blurred one. For this there is no remedy but cutting down the aperture.

Ladies and Gentlemen, we have now a reasonable knowledge of what a lens is, and I would like to go over to the more interesting part of optics—to the combinations of lenses—such as the telescope, the microscope, and camera, which not only have given us so much pleasure, but have enlarged our knowledge so wonderfully; but this would require more time than we have on hand to-night, and I will not tax your patience any longer, but thank you for the attention you paid to the rather wearisome subject, and for the interest with which you followed the lecture during the evening.

Gramme Machine for Illumination.—From *The Telegraphic Journal*, April 15th, 1876.—Experiments in electric lighting have been continued at the Northern Railway Station, Paris. We hear that the Northern Company will, if these trials are satisfactory, light the arrival shed by electricity. This shed has a cubic space of about 300,000 cubic metres. For this purpose four electric lights will be used, rather more powerful than the one used in the luggage rooms and the custom house, which is employed from five o'clock till midnight. At the trials of which we speak a Gramme machine was being used, giving a light equal to 100 jets of gas, consuming 150 litres an hour. Lately, experiments with the photometer and dynamometer have been carried on at the lamp factory of Messrs. Sautier and Lemmonnier. The result of the trials show that this powerful Gramme machine gives a light equal to 1,850 Carcel burners. The equivalent consumption of oil would be (reckoning 40 Grammes an hour per burner) 71 kilogrammes an hour; of gas 194 cubic metres, say about 650 kilogrammes of coal. The cost of electric lighting is about one-hundredth part that of oil.—*Moniteur Industriel Belge.*